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ABSTRACT

This journal details the results of a symposium created to address the question, "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?" Moderators Gary Cronkhite and Jo Liska provide both an introduction and a summary of the three position papers that form the basis of the text. Position papers include "Evidence Varies with Theoretical Perspective" by B. Aubrey Fisher, "Evidence Varies with Phases of Inquiry" by Thomas M. Scheidel, and "Interpretation and Evidence" by Jesse G. Delia and Lawrence Grossberg. Responses by the authors to each other's contributions conclude the symposium. (KS)

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Journal of Speech Communication

Featuring a special symposium on:

"What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?"

Moderators: ***Gary Cronkhite*** **and** ***Jo Liska***

Participants: ***B. Audrey Fisher, Thomas M. Scheidel,*** **and** ***Jesse G. Delia*** **and** ***Lawrence Grossberg***

Speech Communication Association

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Editor's Note

One of the revelations of my tenure as editor of *WJSC* has been the discovery of the degree to which scientifically oriented scholars disagree on basic methodological issues. Investigation of these differences convinced me that they derive from philosophical commitments, not just procedural or statistical preferences. On the assumption that it would be useful to explore these matters for non-scientifically oriented researchers as well as those directly affected, I contacted Professors Gary Cronkhite and Jo Liska with a proposal to serve as guest editors of a symposium of papers that would examine a set of issues I had devised. While they thought the project had merit, they gently informed me that the questions I suggested could be reduced to one—the one that is addressed in the papers that follow. I am most pleased with the result and thank them and their conferees: B. Aubrey Fisher, Thomas M. Scheidel, Jesse G. Delia, and Lawrence Grossberg. I hope the contributions of all of the participants will be considered carefully and their statements will generate further dialogue which can be pursued in future issues of this journal.

WALTER R. FISHER

Introduction

GARY CRONKHITE AND JO LISKA*

AS ONE OF US is fond of saying, the early history of communication research, like the history of most sciences, is a history of the invention and destruction of myths. This is no slur on the discipline of communication or any other science. Indeed, it may be a description of the essence of science. Today's scientific "laws" grow in soil fertilized by yesterday's myths, and these so-called "laws" will inevitably take their places on the compost heap when they cease to be otherwise useful. A science becomes decadent when it takes its own explanations too seriously and turns in pathological fascination to examine and worship its own intellectual droppings.

Thus the most dangerous myths are those which are too enduring, enduring because they are tautological and therefore necessarily true, dangerous because they encourage fixation and stagnation without providing any really useful explanation. A useful myth is one that makes testable predictions and thus, like Oedipus' father, carries within it the seeds of its own destruction. The most dangerous myth is the one that provides a simple label for a variety of complex phenomena, provides a sort of semantic placebo, and endures because of its inherent circularity.[1]

This process of science, so considered, is a special case of the social evolution of ideas as originally described by Campbell.[2] Campbell posits that any evolutionary process requires variety, selection, and retention. We have no lack of *variety* in the study of communication: variety in assumptions, methodology, techniques, data, and theoretical explanations of the data. Too frequently, however, the *selection* process is rather haphazard. Lines of research are pursued in an area because initial findings are contrary to "common sense" expectations, because advocates of that line of research are artistic and imaginative in their speculations, or because such advocates happen to command a corps of graduate students they can unleash on the problem. Once selected in these ways, lines of research are *retained* in journals and on convention programs by virtue of precedent.

Numerous philosophers of science have described this process as one of

*Mr. Cronkhite is an Associate Professor and Ms. Jo Liska is a Lecturer at the University of California, Davis.

[1] Gary Cronkhite, "New Myths for Communication," in *Proceedings of the First Annual Fresno Colloquium on Communication Theory*, ed. David Natharius (Fresno, California: California State University, Department of Speech Communication, 1974), mimeograph.

[2] Donald T. Campbell, "Ethnocentric and Other Altruistic Motives," in *Nebraska Symposium on Motivation*, ed. D. Levine (Lincoln: Univ. of Nebraska Press, 1965), pp. 306-07.

argumentation and refutation conducted by certain rules. In a provocative article in a recent issue of this journal, Wander has taken this metaphor seriously, applying the tools of rhetorical analysis to the scientific process.[3] This conceptualization of science as ideological evolution proceeding by means of argumentation and refutation is interesting in any science, of course, but it is especially interesting in a science which purports to study the process of influence to discover that the *object* of its study is also the *means* of its study. That is, it should be especially interesting to us, concerned as we are with argumentation and social influence, to apply our understanding of the process of influence to that process as it is exemplified in research in our own discipline, thus becoming metacritics of our own scientific dialogue.

Argumentation is distinguished from popular persuasion in that it operates on the basis of certain rules to which the participants subscribe. In legal argument these rules are specified by law and by precedent. Attorneys and judges spend years learning what types of evidence are admissable under what circumstances. A jury will be instructed as to the rules of evidence relevant to the case in question, and will be protected from and denied access to evidence considered to be irrelevant, inflammatory, or otherwise prejudicial. High school and college debaters conduct their argumentation by a series of rules which are less frequently written but still understood with varying degrees of specificity and clarity by the debaters themselves and by their coaches and judges. Rules in argumentation are designed to make decisions more critical and reliable.

The question is, in essence: in the process of scientific argument, what are or should be the rules by which evidence can be judged admissible or inadmissible? If we adopt Rudner's distinction between *method* and *technique*, these rules are *methodological* in nature:

> For the methodology of a scientific discipline is not a matter of its transient techniques but of its logic of justification. The method of science *is*, indeed, the rationale on which it bases its acceptance or rejection of hypotheses or theories.[4]

Students of communication frequently take "methods" courses, but what they study in such courses is more likely to be "specific techniques of observation, or of experimental control. . . ."[5] They may learn the underlying methodological assumptions obliquely, by implication, but they are seldom made explicitly aware of such assumptions.

On the other hand, students of communication sometimes take courses titled "Philosophy of Science" in which they confront methodological assump-

[3] Philip C. Wander, "The Rhetoric of Science," *Western Speech Communication*, XL (1976), 226-35.

[4] Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 5.

[5] Rudner, p. 5.

tions. But unless that course is taught by a person in the field of communication, the assumptions are likely to be those of science in general, or at best those of the social sciences in general. What is lacking is some consideration of what methods, what "logics of justification," what assumptions, what rules of argumentation, are specifically appropriate to the study of communication. For, as Northrup puts it:

> Scientific inquiry in any field must begin not with some method taken over a priori from some other field, but with the character of its own field and the analysis of these problems. A subject becomes scientific not by beginning with facts, with hypotheses or with some pet method brought in a priori, but by beginning with the peculiar character of its particular problems.[6]

We need to make another distinction to clarify the question at issue. Earlier, we wrote of "scientific myths," by which we meant scientific *metaphors*. Scientific metaphors are of two types: *epistemological* and *explanatory*. An explanatory metaphor is one that seeks to make a generalization about a particular data domain, a generalization which encompasses all the available data and from which can be derived testable predictions regarding future or at least potential data. An explanatory metaphor is a theory in the usual sense of that term.

An epistemological metaphor, on the other hand, is much more general: it involves a set of assumptions about how one "knows" the world or, if "knowledge" is specifically denied, as in the case of skeptical or agnostic metaphors, then at least it is a set of assumptions about the significance of the sensory impressions we confront from instant to instant. These epistemological metaphors are the "methods" to which Rudner refers, and the central question of this symposium can be viewed as asking what epistemological metaphors are especially important in the study of communication.

One ought not to choose epistemological metaphors or assumptions capriciously or arbitrarily, because the assumptions one makes are likely to determine the evidence one accepts and, indeed, the research questions one asks. Yet it is absolutely necessary to begin any search for knowledge on the basis of certain assumptions. Even if one absolutely refuses to adopt any unproven assumptions, one ultimately confronts the most basic question of the origin of the material universe. At this point, one can operate on the basis of one of two assumptions, neither of which is logically tenable or even conceivable. One can assume that the material universe has existed "forever," "from eternity." But who can actually conceive of such a thing? On the other hand, the alternative is to assume that the material universe came into being by spontaneous generation out of an absolute void, which is equally inconceivable.

The point is that one must begin somewhere, with some unproven and

[6] F.S.C. Northrop, *The Logic of the Sciences and the Humanities* (New York: Meridian Books, 1959), p. 274.

unprovable assumptions. One's basic assumptions are not subject to proof by reference to prior assumption; the only acceptable evidence for their superiority is the quality of what they produce. And "quality" is, of course, a slippery term. Consequently, as the participants in this symposium attempt to outline the criteria they believe should be used to determine the admissibility of evidence in support of theoretical propositions regarding communication, we must bear in mind that they themselves are operating on the basis of epistemological assumptions which cannot be proven or disproven, only supported. Humility is an important quality in such an endeavor, not merely because it is a more pleasant personal characteristic than is arrogance, but more importantly because it is a sign of epistemological sophistication. To argue that one's assumptions are "true" or "right" rather than merely heuristic and internally consistent seems a fair indication of epistemological naiveté. Further, Poincaré argues that confronting epistemological assumptions prevents epistemological naiveté:

> It is often said that experiments should be made without preconceived ideas. That is impossible. Not only would it make every experiment fruitless, but even if we wished to do so, it could not be done. Every man has his own conception of the world, and this he cannot so easily lay aside. We must, for example, use language, and our language is necessarily steeped in preconceived ideas. Only they are unconscious preconceived ideas, which are a thousand times the most dangerous of all. Shall we say, that if we cause others to intervene of which we are fully conscious, that we shall only aggravate the evil? I do not think so. I am inclined to think that they will serve as ample counterpoises—I was almost going to say antidotes. They will generally disagree, they will enter into conflict one with another, and ipso facto, they will force us to look at things under different aspects. This is enough to free us. He is no longer a slave who can choose his master.[7]

SOME DEFINITIONS

Having discussed the importance of the question, it is now appropriate to provide working definitions of key terms.

Criteria

Recognizing that the definition of "criterion" is one which has occupied philosophers dealing with judgment for as many years as there have been philosophers dealing with judgment, and not wishing to appear arrogant to the point of believing that we can clarify their musings, nor wishing to extend the present discussion unnecessarily, we are willing to adopt a straightforward, common definition given in the *Oxford English Dictionary*, viz.: "b. A test, principle, rule, canon, or standard, by which anything is judged or estimated."

[7] Henri Poincaré, "Truth and Experiment," in *Perspectives in Philosophy*, ed. Robert N. Beck (New York: Holt, 1969), pp. 277-78.

Evidence

Again, we could adopt the *OED* definition of "evidence," which reads: "3. An appearance from which inferences may be drawn; an indication, mark, sign, token, trace." The problem that we wish to avoid is defining "evidence" in such a way that the definition itself imposes criteria for the acceptance of something as evidence and, in so doing, so narrows the range of that which is evidence that it in part answers the question, "What is admissible as evidence? Participants in this symposium are, in effect, writing extended definitions of the term "evidence." For example, does the statement "Feelings are facts" mean that feelings are *evidence*? Our answer is that they *may* be. They are "appearances from which inferences may be drawn." Whether they are "inadmissible as evidence in support of theoretical propositions regarding communication" is a question to be dealt with in the symposium, not one to be decided by definition in an introduction. Further, evidence need not be conceived as limited to "raw data" or "facts," whatever those may be. We grant the status of *potential* evidence to any proposition capable of being cast into the form "It appears that. . . ," and we will depend on the symposium participants to narrow that definition as they see fit.

Theoretical Propositions

Again, this definition could impose limits that we do not wish to impose on the participants. The word "proposition" hardly requires definition, but what is a "theoretical" proposition as compared to a "nontheoretical" or "atheoretical" proposition? We are going to adopt a definition from an unlikely source, the *Dictionary of Behavioral Science*.[8] It seems unlikely because we would expect a definition from such a source to require empirically testable propositions. In fact, however, the definition is much broader: "A theory is a system of hypothetical statements concerning a certain area of scientific inquiry." Thus our only requirement is that "theoretical propositions" be part of a system of propositions, systematic in that they are somehow related to one another. That is, a proposition which is unrelated to other propositions will be considered atheoretical. Other limitations are imposed by the participants, however. Our personal preferences would be to impose more restrictions before granting the status of "theory" to a system of propositions, but this is the definition we will adopt *for the purposes of this symposium.*

Communication

Obviously we must mention this term among our definitions, but we have no hope of producing a definition which would satisfy even the participants, let alone the readers, of this symposium. We would like to limit the discus-

[8] Benjamin B. Welman, ed., *Dictionary of Behavioral Science*, (New York: Van Nostrand Reinhold, 1973), pp. 383-84.

sion to *human communication,* so we will modify S. S. Stevens' well-known definition to read "Communication is the response of a human to a stimulus produced by a human." Even that broad definition may prove too restrictive for some, but we believe it is one that is suitable for our purposes.

The question posed to the participants in this symposium was: "What criteria should be used to judge the admissibility of evidence to support theoretical propositions in communication research?"

Evidence Varies With Theoretical Perspective

B. Aubrey Fisher*

To some extent the question guiding this symposium begs the question. It assumes that communication research is intended to support theoretical propositions regarding communication theory. That communication research should be predicated on the purpose of supporting theoretical propositions is axiomatic. That it does in practice is problematic. Contemporary practices in performing communication research indicate the vital importance of the present question. The admissibility of evidence to support communication theory is crucial to the development of communication theory and, more importantly, to the very viability of the academic discipline. The question needs to be addressed directly and without defensiveness.

Despite the tendency to use the catch-phrase "communication theory," the progress of communication research is largely atheoretical. Such an assertion is based, not on the familiar grounds that communication lacks or needs a theory, but that an awareness of theoretical development does not characterize the bulk of communication research today. Rather, contemporary research in communication proceeds from a primarily empirical orientation. We devise and perform studies, not from a conscious desire for theoretical development, but from a rationale developed from previous empirical studies. The methods and the techniques of empiricism, the nitty-gritty of doing research, become the principal if not the sole means for evaluating our research efforts. To say, then, that communication research is atheoretical is to imply that communication research proceeds from an empirical orientation at the sacrifice of a theoretical orientation.

One symptom of the atheoretical nature of communication research is the tendency to fail to discriminate between variables and theories. Our journals are replete with references to variables elevated to the status of theory—for example, "self-disclosure theory," "ego-involvement theory," "source credibility theory," "leadership theory," "interpersonal attraction theory." Each variable is a concept which certainly may be a part of a theory by being related with other concepts in the form of a theoretical proposition. And those propositions, taken together, provide an explanation for some phenomenon (in the present case, human communication) which can be called a theory. The tendency to grant variables such omnipotent status, however, clouds the

* Mr. Fisher is Professor of Communication at the University of Utah.

theoretical issues in favor of the variable. We rub our favorite variable(s) against other variables, singly or multiply, and thus confuse further the theoretical orientation in favor of an empirical orientation.

The result is an accumulation of mountains of empirical "information" but little cumulative theoretical "knowledge." The jargon of communication has undergone massive proliferation. The difference, for example, between "stagefright," "reticence," and "communication apprehension" is not so much theoretical (different concepts) but empirical (different operationalizations of the same concept). Moreover, communication scientists do not contribute maximally to cumulative knowledge in that they do not share a standardized set of concepts or categories. Our scientific community of communication scholars seems to be composed of cliques identifiable by a specialized "pet" concept, whether it is communication effectiveness, interpersonal confirmation, communication competency, interpersonal trust, a specific set of categories for interaction analysis, and so on.

Even the same terms are not standard across the scientific community. A particular term used by one person may refer to something quite different from the referent for that term used by another. For example, depending upon who uses the term, "system" may refer to cybernetics, structural-functionalism, equifinality, networks, or any of a number of different concepts. Sometimes the term "system" refers to an analytical technique, such as causal modelling or Markov processes, rather than a theoretical orientation. A further example of nonstandardization of concepts is the popular term "transaction," which may imply merely two-way transmission of messages or personality matching or attitude similarity or relational interaction.

One explanation for our empirical orientation to research may be the culture of the university community, the context in which most communication research is performed. The university community tends to foster a reward system which encourages present research practices. That reward system is not merely the publish-or-perish syndrome but a system that fails to reward or encourage theoretical development or development of an ongoing research program. Procuring grants is of paramount importance to many university administrators. Moreover, one criterion for assessing the worth of a grant is its dollar amount. Scholars are then encouraged to choose studies and research problems on the basis of grant money which is available, emphasizing the interests of funding agencies rather than their own. Sublimated is the scholar's own goals or desires to seek an answer to some theoretical research question. Our priorities are often based upon the popularity and probable acceptance of studies and even techniques by reviewers of grant proposals, convention papers, and journal articles.

Underlying this answer to the symposium's question concerning the admissibility of research evidence, then, is the assumption that current com-

munication research is atheoretical. Therefore, the phrase "to support theoretical propositions" is deemed to be of principal importance. The criteria to be suggested will reflect strongly this assumption.

Addressing the question concerning the admissibility of communication research evidence or data, several issues come immediately to mind. The first is the identity of such data. That is, what do communication data look like? What constitutes communication data? For that matter, what does not constitute communication data? The field of communication is extraordinarily broad and includes such data as linguistic units or categories, morphemes, phonemes, markings on a paper-and-pencil test, large-scale social events, media advertising, cinematographic placements, and so on. This issue of identifiable characteristics of data seems unanswerable and probably quite trivial in and of itself.

Other issues might concern the collection of data. For example, where should communication data be collected? Again the issue appears trivial. Wherever communication can and does take place seems an appropriate location for data collection. The issue of field vs. laboratory studies is equally trivial and outdated as an answer to the symposium's question. While either location may generate data of questionable validity or reliability in specific circumstances, the field-laboratory issue is idiosyncratic to data-collecting procedures and should not serve as a blanket indictment of either setting concerning the admissibility of those data.

How should data be collected? This issue is a methodological one rather than theoretical. It addresses the question of superiority and appropriateness of a particular research method—for example, critical, experimental, qualitative, historical, descriptive, ethnomethodological, participant-observation, and so on. Although there may yet be some member of the scientific community who still believes in the myth that some research method is superior to another, Kaplan put that issue to rest some years ago.[1] The crux of Kaplan's argument concerning methods can be summarized by his statements:

> I think it is hard to improve on P. W. Bridgman's remark that 'the scientist has no other method than doing his damnedest. . . .' The more realistic danger is that some preferred set of techniques will come to be identified with scientific method as such. . . . The fragmentation of a science into 'schools' is by no means unknown even in as rigorous a discipline as mathematics; what is striking in behavioral science is how unsympathetic and even how hostile to one another such schools often are. . . . All of them are right; what is wrong is only what they deny, not what they affirm.

Any of the variety of research methods used in communication research is not only appropriate but potentially valuable to communication theory. For

[1] Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), pp. 18-33.

a specific research question, one method may be preferred as more fruitful than another, but no one can deny the appropriateness and value of any research method in other circumstances. Science cannot and does not dictate the use of any method as superior to another. Nor does scientific theory. The issue of which method should be used to collect communication data remains open. Evidence from any method is eminently admissible.

A fourth issue of data collection, however, raises once again the principle of theoretical implication. What is the source of communication data? Do such data reflect externalized behaviors, inferred perceptions, phenomenological introspections? Such questions are absolutely unanswerable except from some theoretical orientation which views the relationships among concepts within some explanatory framework. Specifically, for example, does communication comprise internalized perceptions or externalized behaviors as the principal components of a communicative event? The only possible answer to this question would be phrased within some theoretical framework which serves to explain the communicative event. And that theoretical framework could answer the question in favor of either alternative—behaviors or perceptions—depending upon the theoretical perspective used.

The undisputed fact is that communication as a field of scholarly inquiry is not characterized by a single paradigm in Kuhn's sense of the term.[2] As a "scientific community" of practitioners and researchers, communication scholars have nothing approaching near unanimity of opinion toward or practice in the conducting of communication inquiry. The "disciplinary matrix" of communication is not unified in terms of theoretical structures, methodologies, terms, operationalizations, concepts, or analytical techniques accepted and utilized. We often use the term "communication theory" as though it represented some unified body of literature or beliefs characterizing our conceptual orientations and our empirical practices. But, of course, it does not.

A more accurate representation of the field of communication is that of a set of *competing* paradigms or perspectives. These perspectives compete in the sense that each orientation reflects the practices and beliefs of some discernible segment of the scientific community, but they cannot be easily included under a single umbrella of communication theory implying a unified rationale. Feyerabend,[3] as well as Lakatos and Musgrave,[4] would consider

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[2] See: Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: Univ. of Chicago Press, 1970); and Thomas S. Kuhn, "Second Thoughts on Paradigms," in *The Structure of Scientific Theories*, ed., Frederick Suppe (Urbana: Univ. of Illinois Press, 1974), pp. 459-82.

[3] Paul Feyerabend, "How to Be a Good Empiricist—A Plea for Tolerance in Matters Epistemological," in *Philosophy of Science: The Delaware Seminar*, Vol. II, ed., B. Baumrin (New York: Interscience, 1963), pp. 3-40.

[4] Imre Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge Univ. Press, 1970).

these competing and often conflicting paradigms or research programs as symptomatic of a healthy discipline. Whether competing theoretical perspectives are desirable is a matter of philosophical dispute, but it is the state of affairs with which communication scholars must come to grips in order to perform productive research.

One method of classifying the competing perspectives in communication theory is to focus on the structural aspects of the theory itself. Such a focus might generate theoretical structures such as covering-law theory (or hypothetico-deductive or axiomatic), rules theory, open-system theory, or cybernetic theory. The 1975 SCA convention program oriented to these theoretical approaches revealed considerable competition among these perspectives as well as competition in how each approach might be applied to communication theory. In other words, one person's system-theory approach to communication, for example, may be quite different from and conflict with another person's system-theory approach.

Perhaps a more valuable approach to classifying competing perspectives in communication theory is to focus on the applications to and classifications of communicative phenomena. Such applications imply an episteomological classification. Briefly, such a list might include mechanism, psychologism, interactionism, and pragmatism as perspectives characterizing contemporary communication research and speculation. These perspectives differ from each other in terms of identifying primary variables, relating variables to each other, operationalizing similar concepts—in short, epistemological rather than structural or syntactical considerations.

A mechanistic perspective centers on the transmission and receiving of messages along a channel with a corresponding emphasis on the transitivity of communication effects. Such a perspective has become susceptible to numerous attacks. Black, for example, criticized the implicit mechanism in rhetorical criticism calling it "neo-Aristotelianism."[5] Among others, Bauer attacked the mechanism implicit in "effects studies" as failing to take into account the "obstinate audience."[6] The result was a tempering of the mechanistic perspective with a psychological perspective—depicting the active receiver capable of selectively attending to and perceiving messages and even distorting incoming stimuli to be consistent with internalized mediators. A psychological perspective then focuses on the receiver as an active processor of information. As a result of the psychological orientation, some rhetoricians turned to Chaim Perelman's receiver-centered approach. Other scholars reoperationalized the concept of ethos as emanating from the source to the concept of credibility as perceived by the receiver. Researchers in media effects

[5] Edwin Black, *Rhetorical Criticism: A Study in Method* (New York: Macmillan, 1965).

[6] Raymond A. Bauer, "The Obstinate Audience: The Influence Process from the Point of View of Social Communication," *American Psychologist*, 19 (1964), 319-28.

revised their attempts to discover how the media affect an audience to inquiries into how an audience uses the media and what characteristics in the audience are susceptible to influence by mediated stimuli.

An interactional perspective of communication also grew out of an anti-mechanistic bias and attempted a more humanistic approach to communicative events. While yet a fragmented perspective, interactionism can be traced in part to the influence of symbolic interactionism. An interactional perspective of communication elevates the social actor to prominence and depicts the communicative event as a cooperative search for interpersonal "communion and the ground of self-discovery."[7] This perspective serves to unite within a relatively common perspective such diverse elements as phenomenologists, many symbolic interactionists, ethnomethodologists, dialogists, and so on. The emphasis of such a perspective is on the empathic ability of the communicating individual to achieve some communicative relationship and social meaning during the communicative event.

A pragmatic perspective springs from Watzlawick, Beavin, and Jackson's application of open-system theory to communication.[8] It focuses on the social structure established over time as revealed in the redundancy of sequences of externalized behaviors. Such a perspective emphasizes the patterns of interactions or behaviors analyzed by some form of stochastic probability (e.g., Markov chains, information-theory statistics) and de-emphasizes the importance of internalized perceptions of the individual actor as variables which serve to explain subsequent behavior.

Returning to the source-of-data question concerning externalized behaviors versus internalized perceptions allows for a comparison of these theoretical perspectives. Functioning within a mechanistic-psychological perspective, one might argue that the nature of the relationship between perceptions and behaviors is a significant question for inquiry. Either it is true that perceptions guide behaviors or that behaviors generate subsequent perceptions or that perceptions and behaviors mutually influence each other. The issue has been the subject of a continuing controversy and has received its most noticeable expression as the attitude-behavior discrepancy.[9]

An interactional perspectivist might argue, on the other hand, that behaviors are the objects of perceptions. That is, the communicator attempts to role-take "others" in the sense that he or she infers and assimilates other's beliefs or behaviors during the process of interaction or communication. Behaviors are thus overt manifestations of role-taking and are the objects

[7] Floyd W. Matson and Ashley Montagu, eds., *The Human Dialogue: Perspectives on Communication* (New York: Free Press, 1967), p. viii.

[8] Paul Watzlawick, Janet Helmick Beavin, and Don D. Jackson, *Pragmatics of Human Communication* (New York: Norton, 1967).

[9] See, for example, David R. Seibold, "Communication Research and the Attitude-Verbal Report-Overt Behavior Relationship: A Critique and Theoretic Reformulation," *Human Communication Research*, 2 (1975), 3-32.

of self perceptions. In fact, one's own behavior is the object of internalized perceptions for the purpose of assessing one's own behavior.[10] Behaviors, then, are the objects of perceptions so that perceptions guide further perceptions and use behaviors as evidence for those perceptions.

From a pragmatic perspective one can argue that the only way for any person to affect another person in a social setting is through behavior—observable and externalized. Thus, according to Weick, "The collective structure can involve only those behaviors that A and B are capable of producing. A collective structure exists when behaviors of two or more persons become interstructured and repetitive. The unit of analysis now becomes the interact or double interact and *not* the act."[11] Hawes elaborates on this association with internalized perceptions and externalized behaviors in terms of his postulates of concatenaity, simultaneity, and funtionality implying that perceptions either follow and are validated by behaviors or that perceptions proceed from patterned behaviors.[12] In either case, perceptions by themselves are less significant than behavioral patterns.

To answer the question, "What is the source of communication data," then, requires placing the specific study within a theoretical, *not* an empirical, framework. If the particular study serves to support a theoretical proposition within a mechanistic-psychological perspective, the data could be either perceptual or behavioral. Interactionally, data would probably be perceptual. But if the study attempts to support a theoretical proposition within a pragmatic perspective, the data should be behavioral. Consequently, the first and by far most important criterion to judge the admissibility of evidence to support theoretical propositions in communication research is: is the evidence consistent with the conceptual/theoretical rationale used to guide the research.

Such a criterion implies several corollaries which appear intuitively obvious. First, the researcher must be aware of the theoretical perspective underlying a research effort. That awareness should be demonstrable by a rationale which articulates clearly the theoretical orientation. The typical practice of atheoretical research in communication is to provide only an empirical rationale—a list of previous research studies investigating the same variables. The theoretical rationale is thus slighted. It should not seem too much to ask of editors and reviewers to demand that the researcher articulate the theoretical rationale or perspective which guides the study. Such an articulation is clearly not in terms of the empirical rationale or of the yariable

[10] See, for example, Robert L. Kelley, W. J. Osborne, and Clyde Hendrick, "Role-Taking and Role-Playing in Human Communication," *Human Communication Research*, 1 (1974), 62-74.

[11] Karl E. Weick, *The Social Psychology of Organizing* (Reading, Mass.: Addison-Wesley, 1969), p. 46.

[12] Leonard C. Hawes, "Elements of a Model for Communication Processes," *Quarterly Journal of Speech*, 59 (1973), 11-21.

labelled as a "theory," but the theoretical perspective of communication theory, taken as a whole.

Then, too, the researcher should be aware of the theoretical perspectives which are *not* being utilized to guide the study. A good working knowledge of all theoretical perspectives available is the best insurance against asking trivial research questions. Each perspective has its advantages and its limitations. And the perspective chosen to guide the specific study is precisely that—a choice. And a choice made with complete knowledge of all alternatives is not only the most rational choice, it also maximizes the likelihood of selecting a research question which will lead to theoretical development. Choosing the research question without conscious thought of the theoretical perspective is like buying a new automobile without being aware of the various makes and models available—intellectual naiveté.

Once the particular research study is identifiable within its conceptual/theoretical rationale, other criteria to judge admissibility of research evidence take on secondary importance but are nonetheless significant. Are the data consistent with the research question? Unfortunately it is not uncommon to read a report of research asking a question concerning communicative behavior but using perceptual data to answer the question. For example, if the investigator wishes to identify the communicative behaviors of group leaders, the evidence to support that research question should be in the form of the behaviors performed by the leaders, not the perceptions of the followers as to what they perceived the leader to be doing. In other words, if the question seeks to identify leadership behaviors, the investigator should look at what the leader does, not ask other group members for their perceptions. This criterion ultimately comes back to the theoretical rationale. The question concerning leadership appears to emanate from a pragmatic perspective, but the research evidence seems more consistent with a psychological perspective.

A third criterion might address directly the role of communication in the research study. A few years ago I conducted a "quick-and dirty" survey of published communication research in order to discover the placement of communication in the research design.[13] To no one's surprise I found the majority of studies considered communication to be an independent variable. Of more concern was the surprising number of reports that did not include communication in the design at all. Consistent with McGrath and Altman's criticism of small group research of a decade ago, communication may be gravitating toward becoming a "science of the independent variable."[14] More important is the potential neglect of the dependent variable side of communication. Knowing the effects and outcomes of communication is important, to

[13] B. Aubrey Fisher, "Paradigms of Communication," a paper presented at the Western Speech Communication Association Convention, Newport Beach, California, 1974.

[14] Joseph E. McGrath and Irwin Altman, *Small Group Research: A Synthesis and Critique of the Field* (New York: Holt, 1966), p. 75.

be sure. But research must not ignore an understanding of the characteristics of communication, how it works, what behavior means, and so on.

Of more concern to the field of communication, however, is the study that does not include any communicative event whatsoever. Occasionally such a study will include a brief description of some hypothetical situation involving communication and ask for subjects' reactions to it. And there are also studies in our journals which provide no communicative event beyond that of the investigator's instructions to the subjects on how to fill out the scales. Surely, if communication is a viable area of research as an academic discipline, the role of communication should be the central focus of any study purporting to be communication research.

Are the data, the analysis, the conceptual orientation supported by rational argument justifying the worth of the question, its position in the theory, its contribution to furthering theoretical development? This final criterion admittedly borrows from a staple ingredient of critical research reports. The critic must always argue for the worth of his/her method and how the results from the critical inquiry were obtained, including the extent of their significance. The empiricist typically does not. Rather, he/she assumes that the presumption favors the specific methods or techniques utilized in collecting and analyzing the data. Kaplan argues strongly against any presumption favoring the specific techniques used in conducting research.[15] He contends that such techniques come to be "so mechanically applied that it [the techniques] undermines the very spirit of scientific inquiry." He states further "it is not *they* that produce scientific results but the investigator who uses them scientifically." Kaplan's point is well taken. Any presumption should favor the conceptual worth of the question, regardless of the techniques employed.

The problem underlying the use of certain methodological formulas in the empirical orientation of communication research is not a condemnation of any particular method. Rather, the problem is a tendency to use some methods as formulas which implicitly are, in and of themselves, worthy of justifiable research. But methods and techniques are simply instruments to be used for any purpose—significant as well as insignificant. Phil Emmert stated in a critique of convention papers several years ago that he preferred the meaningful but less well done research to the "well done but meaningless" study which is all too common.[16] I could not agree more.

Specifically Emmert was attempting to emphasize the overwhelming significance of the theoretical rationale underlying the research. He states, "a necessary ingredient in the report of a study should be a theoretical justification and explanation of the study being reported. . . . It is not enough to

[15] Kaplan, pp. 28-29.

[16] Philip Emmert, "Theoretical Implications," a critique of papers presented at the Western Speech Communication Association Convention, Honolulu, 1972.

say that there have been fifteen previous studies and that they were not adequately conducted; it must be demonstrated that a theoretical vacuum exists and can be filled by the study of the variables and their relationships as you have done." Emmert places the inevitable rationale for any scholarly investigation where it belongs—the conceptual/theoretical orientation, and not merely a survey of past empirical studies. Expecting that orientation to be explicit in the report of the research is eminently reasonable. If theoretical development of communication is a desirable goal and probably an ultimate goal, that explicit rationale is essential.

In summary, then, the criteria I would recommend in response to the question posed in this symposium include the following:

1. Is the evidence consistent with the conceptual/theoretical rationale guiding the study?
2. Is the evidence consistent with the research question?
3. Is the evidence consistent with a central focus on human communication?
4. Is the evidence, the analysis, and the conceptual/theoretical orientation supported by rational argument which justifies the worth of the question, its position in the theory, and its contribution to theoretical development?

Clearly the first criterion is the most significant. And the remaining three criteria ultimately depend on the first.

While it is conceivable that other criteria could also be employed to judge the admissibility of evidence in communication research, it is vital that such criteria avoid what Feyerabend calls "arguments from synonymy."[17] According to this maverick philosopher of science, "Arguments from synonymy judge a theory or a point of view not by its capability to mimic the world, but rather by its capability to mimic the descriptive terms of another point of view which for some reason is received favorably." An argument from synonymy thus judges research evidence, not on the basis of its theoretical contribution, but on the basis of its similarity with popular research practices. But to require an axiomatic theory of communication, for example, is no more valid than to require a rule-conforming theory. To require that all research evidence be composed of numbers and all analyses be statistical is no more valid than to require all evidence be critical reflections and all analyses be phenomenological. Any research evidence can be judged only within a conceptual/theoretical framework. And that framework can be judged only by its ability to contribute to our knowledge of the world. Judging any research evidence or theory on the basis of its popularity among members of the scientific community leads directly to disciplinary dogmatism and sterility. It is intellectual arrogance at its worst.

[17] Feyerabend, p. 32.

B. AUBREY FISHER

I often tend to be impatient with soul-searching explorations into communication theory and research. I am tempted to say enough is enough. Let's get on with the business of doing research. But that impatience is naively short-sighted. Addressing directly issues such as the ones raised by this symposium are absolutely essential to a developing and healthy discipline. It is also comforting to know that other fields, notably sociology and psychology, have undergone similar introspections in the past decade. Constant reassessment and stock-taking can be but beneficial in the long run. The only alternative is empirical and theoretical myopia. And at this stage of development, communication theory needs 20-20 vision.

Evidence Varies With Phases of Inquiry

THOMAS M. SCHEIDEL*

> on the face men hang as it were, and fix their gaze and entire attention on it, even before we begin to speak, by the face we express love and hate; from the face we understand numbers of things, and its expression is often equivalent to all the words that we could use.
>
> —Quintilian

> It is suggested that the combined effect of simultaneous verbal, vocal, and facial attitude communication is a weighted sum of their independent effects—with the coefficients of .07, .38, and .55, respectively.
>
> —Mehrabian

> We know a person's face, and can recognize it among a thousand, indeed among a million. Yet we usually cannot tell how we recognize a face we know. So most of this knowledge cannot be put into words.
>
> —Polanyi

WE ARE TRAINED from our earliest years to accept without much question the assertions of authorities. Mother and father are all. And we learn early to learn from our own experience. Seeing is believing. To make things easier yet we learn to see and believe what we like and find comfortable to see and believe. These lessons of youth are not soon or easily forgotten. I would argue that these factors play a much greater role in the entire scientific enterprise than we might readily admit.

Evidence—What Evidence?

So Quintilian wrote that facial expression is often equivalent to all the words that we could use. What was his evidence? He did not give us the N of his S's but he clearly was an empiricist. He illustrated his arguments with references to speakers of the day in addition to historical references to Greek and Roman speakers, writers, and actors. But it seems to me that his assertions rely largely upon his own authority. He was himself a successful pleader and speech teacher and writer. His works are still in print (which is more than we can say for some). In addition to his own authority, he has our experience in his favor. So long as we do not raise too many questions about the reliability and validity with which the face is perceived to "express

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love and hate" and we do not inquire too closely into how one might balance "facial expression" against "all the words that we could use," we may admit that our experience generaliy confirms his assertion. It must be obvious, however, that we are some distance from the evidence upon which *he* based that claim.

Mehrabian, too, is an authority. His assertion is so clear and specific that it can hardly be questioned. And it seems hardly to be questioned. The following examples illustrate how his claim has been taken up and made even more general.

> Most researchers agree that the nonverbal channels carry more social meaning than do the verbal channels.

> they concluded that when you use all three modes—words, tone of voice, and facial expression—words contribute 7%, tone of voice 38%, and facial expression 55% of the total meaning.

Mehrabian's evidence is not so far removed from us as is that of Quintilian. At least we know more about the procedures by which he gathered his evidence. In one of Mehrabian's studies, for example, he chose the single word "maybe."[1] He then made tape recordings of speakers saying "maybe" so as to indicate feelings of liking, neutrality, and disliking toward an imagined listener. Photographs were taken of models as they attempted to use facial expressions to communicate feelings of liking, neutrality, and disliking toward another person. Each recording of "maybe" was paired with each photograph and 20 female undergraduates were instructed as follows:

> The purpose of this study is to find how well people can judge feelings of others. You will be shown photographs of different facial expressions and at the same time you will hear a recording of the word "maybe" spoken in different tones of voice. You are to imagine that the person you see and hear (A) is looking at and talking to another person (B). For each presentation, indicate on the scale what you think A's attitude is toward B.

The scales were modified semantic differential scales; like ..... dislike. From this and a similar study the conclusion was drawn that was quoted above.

When I read Mehrabian I believe he clearly intended some qualification when he wrote "it is suggested." And he did propose that tests be made of his weighted model. I cannot believe that we are yet ready to generalize from a single word to "words" or "verbal channels." Nor do I believe we should generalize from judgments of liking and disliking to "social mean-

[1] See: Albert Mehrabian and M. Wiener, "Decoding of Inconsistent Communications," *Journal of Personality and Social Psychology*, 6 (1967), 109-114, and Albert Mehrabian and Susan R. Ferris, "Inference of Attitudes from Nonverbal Communication in Two Channels," *Journal of Consulting Psychology*, 31 (1967), 248-52.

ings" or still yet to "total meaning." Nevertheless, citations of the 7%—38%—55% formula are as common in the literature today as the assertion of years ago that 70% of our waking day was spent communicating—listening, speaking, reading, and writing.

The problem I would like to identify here is the fact that *we seldom encounter the basic data (call it first level evidence) upon which scientific advance in communication theory is based.* Consider the usual case. A scientific study is completed by a research scholar. Many months later it may be reported in extremely abbreviated form in a research journal. We learn in a paragraph or two how the data were collected and we see some results of the processing of some of those data. We never see the first level data. In the ensuing months and years the summarizing and synthesizing statements from the article are passed along in the publications of others—first in research articles, then in research reviews, finally in textbooks. As the statements are passed along, as in the old telephone rumor game, they show influences of leveling, sharpening, and assimiliation.

So in the end, as in the beginning, we seem to be relying largely upon the authoritativeness of the person making a scientific claim, and upon the ease with which we can assimilate that claim (all of which we could call second level evidence).

When discussing criteria for evidence, the inclination is to become philosophical and to focus on first level evidence. Yet Kaplan reports, "a panel of psychologists recently concluded that 'philosophy of science has little or nothing to do with how research gets done in psychology.'"[2] Kaplan claims that "this conclusion may respond to the fact that the interests of philosophy of science are primarily philosophical rather than scientific."

I would suggest that we must acknowledge how seldom it is that we ever see the first level evidence of others. Perhaps we should do more to get at that evidence. Perhaps more replications should be encouraged in communication research to try at least to duplicate that first level evidence. On the other hand, however, to the extent that second level evidence is the best we have and is influential in communication research, perhaps we should consider and apply more direct standards about other researchers as researchers. Just how knowledgeable is that researcher; what has been the training and what is the special expertise of that researcher; what other than first level evidence might have influenced the conclusions of that researcher? We have been trained so to accept the word of authorities that it is difficult to contemplate applying tests of authority to scientific researchers. One could hesitate, feeling that such evaluation would be seen as a personal attack. Better to look at the evidence (meaning data). But I am arguing that the person of the researcher very often is the evidence.

[2] Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1974), p. 22.

The Scientific Process—An Overview

First level evidence is available to us at least in our own research. What tests are we then to apply? In answering this question, it may be helpful first to present some views about the scientific enterprise as I see it in relation to the development of communication theory. Some scholars distinguish between science as product and science as process. I prefer the view of process and see scientific research as activity rather than primarily as accomplishment. Leonard Hawes shares this perspective and it was inevitable that in presenting a critique of three alternative theoretical bases for the study of human communication at the 1975 SCA Convention he would argue for the need for evidence prior to theory development. He asserted that:

> before we search for covering law theories to answer 'why' questions, let's be certain we have sufficient descriptive and interpretive work for those explanations. We tend to be so enamored with the elegance of covering law theories, we overlook the fact we may have insufficient material and inadequate questions for such theories. Let's begin at the beginning.[3]

With Hawes penchant for the gerundial form in mind, we could label the view of science as process as "Hawesing." I agree completely with his assertion that we should begin with observations of the phenomenon of our interests. To cite an authority, Kaplan observes that we often "tinker with the model when we might be better occupied with the subject matter itself." We must know subject-matter prior to theory.

The definition of theory that I look to most frequently is given by Floyd Allport:

> A theory . . . is a well-coordinated conceptual system. It attempts to give rationale coherence to a body of known empirical laws; and from it can be deduced theorums whose predictive value can be tested, thereby appraising, in part, the validity of the theory. . . . Though a background of observation lay behind them, they were the results of the attempt without further experiment at the moment, merely by *thinking about* what was already known, to put together the pieces of a puzzle. . . .[4]

A theory, then, is a conceptual system that serves to explain some phenomenon. It should be differentiated from a guess, a hunch, or a hypothesis. It must have explanatory power, generality, and be sufficiently operational in its statement so that it can be tested. The development of theory is central to the full scientific enterprise.

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[3] Leonard Hawes, "Alternative Theoretical Bases for the Study of Human Communication: A Response," a paper presented at the Speech Communication Association Convention in Houston, Texas, December, 1975.

[4] Floyd Allport, *Theories of Perception and the Concept of Structure* (New York: Wiley, 1955), pp. 4-5.

Scientific advance can be viewed as the result of three interdependent phases and processes: (1) the careful *observation, analysis* and *description* of the phenomenon in which we are interested, (2) the *development of theory,* or the invention of a conceptual system that provides an explanation and *understanding* of the phenomenon, and (3) the *testing* of specific predictions derived from that *theoretical explanation,* which serves as a check upon our understanding. These three processes are circular and interactive. Theories, then, in part, may be developed from observations and questions about phenomena; they provide explanations and understanding, and they lead to predictions which provide tests of their usefulness. This view simply asserts that scientific advance will result best from the combining of these three processes. Progress in the theory building phase by means of scientific revolution as described by Kuhn could fit well in this scheme.

These three phases overlap, and the overlapping of the first and second phase and of the second and third phase are most interesting. The first overlap includes the theory developing process and the second overlap covers the theory testing process.

This view of scientific advance does not hold that every research project can or should encompass all three of these phases, or that every scientific researcher must be competent in all three. It may even be that inclinations to careful observation, measurement, and description run counter to those leading to theoretical speculation and invention.

To illustrate these phases we could consider the behavior of the pigeons in Skinner's classic conditioning studies. Those pigeons did tolerably well on the first two steps—they accurately observed their periodic rewards, and they seem to have developed an explanation for those rewards. Their actions indicated that they "explained" the observed rewards as being "caused" by their patterns of behavior. They seem to have "believed" sufficiently in this correlational explanation to have adopted behaviors guided by it. Had these pigeons been motivated by a scientific attitude, they would have been led to an experimental manipulation and testing of the "my behavior causes reward" explanation. That explanation would then have been falsified.

The purpose for prediction is primarily instrumental in theoretical development. Prediction provides the means for testing our understanding. Understanding is our goal and that understanding will determine our future actions. A specific prediction, useful for testing a theory, is always narrower than the theory itself. This is one of the reasons why an experimental test of a theory can never validate the theory, but can serve only to falsify it. The pigeons *needed* predictions in order to test their understanding or theory.

I would see the first phase of scientific theory development as presented here to be expansive and to involve data and evidence. The second phase primarily requires mental activity. The third stage is reductionist and nar-

rows again to evidence and to specific tests of the results of that mental activity. The ideal of theory testing in this third phase could be typified in a test of Einstein's theory conducted in the early 1930's. One specific, narrow prediction following from Einstein's grand and complex theory had to do with light waves curving in space. A study was conducted in which the distance between two stars was measured when they appeared on either side of a solar eclipse and the light from them had to pass by the sun. The distance between these stars was also measured without the special influence of the sun. Einstein's theory predicted that the light rays would be bent when passing the sun and, therefore, that the two measures of distance between the stars would differ. This was a very narrow test of a very specific prediction deprived from the very grand theory. The entire theory obviously could not be put to test, but very narrow predictions growing from it could.

John Dewey also described the complete thought process as involving observation in the initial and end stages and as involving mental activity in the middle stage.[5] It must be clear that, while description and observation are essential in the first phase, these data alone do not lead naturally and inevitably to theory. Theory development requires something beyond observation and description. The definition of theory cited earlier got at this point by insisting that theories are "the results of the attempt, without further experiment at the moment, merely by *thinking about* what was already known, to put together the pieces of a puzzle." This point was made in a letter from Albert Einstein to Karl Popper stating, "I really do not at all like the now fashionable 'positivistic' tendency of clinging to what is observable. . . . I think (like you, by the way) that theory cannot be fabricated out of the results of observation, but that it can only be invented."[6] Careful empirical observations are very useful for theory building, but theoretical development will never be fully achieved without the *added* dimension of careful and creative thought.

When considering the major question of this paper with the perspective of the scientific process just explicated, I come to the conclusion that *different standards for evidence are appropriate in the different phases of scientific development.*

Evidence in Phase I

Suppose for a moment that Jacques Costeau or Marlin Perkins of Wild Kingdom, or another researcher whose work is typified by a reliance on obtrusive measurement, were asked to study communication in a family setting. The image that comes to mind is of a number of researchers armed with rifles chasing the family members through the house and shooting them with tran-

[5] John Dewey, *How We Think* (Lexington, Mass.: Health, 1910).
[6] Karl R. Popper, *The Logic of Scientific Discovery* (New York: Science, 1961), p. 458.

quilizer darts. When the family members are incapacitated (and no longer dangerous), they would be weighed and measured and would have small mikes and transmitters attached. The researchers would then depart and would keep recordings of who is where and what sounds each is making. There would be no special questions or hypotheses but rather an unstructured collection of observations. Films could be made and shown with some editing as was the case with the Loud family. Such research would provide *a collection of observations rather than a collection of data.* These observations might have some limited usefulness for other investigators, but they would not in themselves provide rich evidence for developing communication theory.

The research question is all important, because it is that question which provides the direction and the structure for the observations and conceptualization necessary to answer it. As Coombs points out in *A Theory of Data,* our observations must be focused and structured to yield true data (beyond mere observation) and such focusing and structuring will depend largely upon the scientist's questions. The first test of research evidence in Phase I then would be a test of the question it is collected to answer. Since the question relies so heavily on the abilities and creativity of the researcher, we may be driven back once more to tests of expertness, competence, and authority.

We would ask next about the "richness" of the evidence and whether it has high potential value for theoretical development. Krippendorff's paper, "On Generating Data in Communication Research," is helpful at this point.[7] He incorporates Coomb's views in developing the arguments that aggregational and network data fail to capture the relational qualities and processual characteristics of the communication process. Krippendorff argues that communication data must satisfy the following demands.

1. There must be identifiable in time and space two or more component parts of a system.
2. The component parts and consequently the system as a whole should possess dynamic properties jointly and of their own.
3. Such data must provide the basis for deciding whether or not structure is transmitted among the component parts of a system.

He distinguishes between minimum and rich communication data and we are here raising questions regarding the potential richness of communication research evidence. It seems to me that aggregational data could be useful for some questions a communication scholar might ask, but I would agree that too much of our research never does get fully at the process of communiction.

The systems perspective is extremely useful and I would suggest that one standard we could ask of much evidence in Phase I is that it fit the system perspective. Bergmann defined a process as "the temporal unfolding of a sys-

[7] Klaus Krippendorff, "On Generating Data in Communication Research," *Journal of Communication,* 20 (1970), 241-69.

tem." He stated that in order to be able to specify a process we must be able to specify (1) what the system is, (2) what a state of the system is, and (3) the process law which predicts the temporal sequence of states of the system.[8]

In this initial phase in scientific inquiry, the goal must be to gain the fullest possible knowledge about the phenomenon. Without that knowledge, theory development is hardly possible. Good questions and a systems perspective will help. But the proper attitude is also needed. The basic attitude in this phase should be exploratory. The investigation should be broad, involving a wide range of approaches and techniques. It should involve more scanning than focusing. There have been more descriptive, survey, exploratory studies reported recently in the literature. But still there are many studies written as if they provide observations leading to theories, but which are so narrow and focused that they would be much better suited as procedures for testing theories.

It is our argument that in Phase I we should take the fullest possible view of the phenomenon. One can see a part without seeing the whole; the opposite is not the case. Reductionism is not the answer at this point. We must see our phenomenon at the highest levels, because we cannot move easily from the lower levels upward. As Polanyi writes:

> the operations of a higher level cannot be acounted for by the laws governing its particulars forming the lower level. You cannot derive a vocabulary from phonetics; you cannot derive the grammar of a language from its vocabulary; a correct use of grammar does not account for good style; and a good style does not provide the content of a piece of prose.[9]

It seems to me to be possible to relax our standards for evidence somewhat in this first phase as we engage in broad scanning.

Must our evidence always be observable? Must it always be potentially available to others? I would say *no* in Phase I of scientific development but *yes* in Phase III. I believe Polanyi has argued convincingly for "tacit knowing." He claims that "we can know more than we can say" and he illustrates his claim with the case of the human face that is quoted above. He maintains that "our body is the ultimate instrument of all our external knowledge, whether intellectual or practical." He also cites German thinkers of the turn of the century who took the position that we come to know humankind by indwelling or empathy: that we must put ourselves in the experience in order to best understand it.[10]

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[8] Gustav Bergmann, *Philosophy of Science* (Madison: Univ. of Wisconsin Press, 1966), pp. 92-93.

[9] Michael Polanyi, *The Tacit Dimension* (New York: Anchor, 1967), p. 36.

[10] See: Michael Polanyi, *The Tacit Dimension* (New York: Anchor, 1967). See also: *The Study of Man* (Chicago: Univ. of Chicago Press, 1959), and *Personal Knowledge* (Chicago: Univ. of Chicago Press, 1958).

It is obvious that when a number of persons observe the same phenomenon some will see more than others. Some simply are more perceptive and acute. There can be no value at all in ruling out introspection as an aid to research in this phase of the scientific enterprise. I have learned more about the speech communication process from the introspective accounts of certain psychiatrists than I have from many tightly controlled experimental studies.

The best available evidence in Phase I should come from a creative and perceptive person viewing the communication phenomenon broadly, with a systems perspective, and guided by some good questions and good luck. We would hope that the researcher could present as much first level evidence as possible. We would not demand, however, that the researcher would be able to show us or tell us about all the evidence of the study. We can permit some evidence from tacit understanding in Phase I.

Evidence in Phase II

The movement from Phase I through Phase II involves theory building —mental activity. At this point we would apply tests of reasoning, inference, internal consistency, and the standards for theory mentioned earlier. There is little regarding evidence that we must consider directly at this point. In Phase II the creative scholar is given a good deal of free rein—or rope. If a program of research effort yields useful new understanding, then that research will prevail. If not, little harm will be done.

It is a great advantage of empirical investigation that research failures are difficult to conceal. This does not mean that we would expect researchers to recant when they fail. We would not expect them to give up on non-significant findings or "strong tendencies." As Kuhn quoted Max Plank:

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.[11]

Empirical research failures, however, are usually apparent to everyone but the researcher.

So at this stage of theoretical development the concern is much more with creative mental ability and activity and much less with evidence. If Janis postulates that "Groupthink refers to a deterioration of mental efficiency, reality testing, and moral judgment that results from in-group pressures,"[12] we can say "how interesting" and reflect ourselves on how that relates to Freud's treatment in *Group Psychology and the Analysis of the Ego*. Janis' observations about leadership based upon his data seem useful in terms of our own experiences with groups and his causal claim could be put to the

[11] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Univ. of Chicago Press, 1962), p. 150.

[12] Irving L. Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972), p. 9.

test. If Fiedler writes of his contingency model as the "best articulated theory" of leadership, we need not react. We might ask "compared with what?" But we would likely want to learn more about that theory. If Berger takes it as an axiom [a self-evident truth] in his theoretical development that "decreases in uncertainty level [information gain] produce increases in liking" when strangers meet, we would want first to learn more about the reasoning behind the entire theory.[14] This phase is integrative and mental.

There is a possible problem at this stage which relates to some earlier comments. Once a theoretical overview has been offered, and it has passed through two or three recountings, what may have been a "theory in the building" can have become a "theory—tried and true." In this phase we must ask whether the theories developed have adequate generality, agree with the facts, are logically available to prediction and testing. But another major objective here will be to differentiate between theories being developed and theories ready for testing.

Evidence in Phase III

In Phase III the pigeons come home to roost. Here we return again to observation and the need for the most severe tests of evidence. We were interested earlier in checking the observations of others; at this point we are checking the thinking of others. In this phase the movement is toward focusing, narrowing, and testing theoretical understanding. Specific predictions from theory are now put to the test. At this point it seems to me that the standards for evidence beome very different from the standards applied in Phase I or even in Phase II. The building of theory can be the work of one, but the testing of theory must be available for the witness of many. We cannot trust a builder who would also be the sole tester. It would be necessary, then, to accept the word of all who argue from private facts. Because multiple witnesses are necessary, we cannot rely on tacit knowledge in Phase III unless several of us have approximately coincident tacit knowledge. In this phase, we become particular and demanding.

It is when we move to tests of theory in Phase III that I become most uncharitable. Fiedler asserts that:

> The Contingency Model is today one of our best validated leadership theories.
>
> the theory is highly predictive . . .
>
> an average correlation between LPC [Least Preferred Coworker score] and group performance of 0.50 tells us that roughly 25 per

[13] Fred E. Fiedler and M. Chemers, *Leadership and Effective Management* (Glenview, Ill.: Scott, Foresman, 1974).

[14] Charles R. Berger and R. Calabrese, "Some Explorations in Initial Interaction and Beyond: Toward a Developmental Theory of Interpersonal Communication," *Human Communication Research*, 1 (1975), 99-112.

> cent of the variation is accounted for by the leader's LPC score. This percentage is particularly impressive when we consider the complexity of the leadership problem. . . .[15]

But when I peruse the median correlations from all the field, laboratory, and original studies testing the Contingency Model, I get an average correlation of about .30 which accounts for less than 10 per cent of the variance that we are interested in. Of the 23 median correlations summarizing the studies, I see only four of .50 or higher and eleven of .30 or lower. I am not "particularly impressed" with correlations ranging between .50 and .30. I would not call them "highly predictive." In my own work with leaders, I am usually dealing with individuals. I would not want to predict very much for a person based on these correlations if that prediction was to be taken seriously.

Berger's axiom seems to me to imply that as I meet a stranger and gain some information about the person I will begin to like that person. But I am unlike Will Rogers. It may be a character flaw but I sometimes take a dislike to strangers early in a first meeting—sometimes even from the facial expression. Any theory taking that relationship between information gained and liking as a self-evident truth has some way to go to convince me.

Both Fiedler and Berger will need more than their authority to convince me. And my own experience does not help their claims. I will not accept their tacit knowledge. I would like simple and straightforward predictions from their theories (better yet if those predictions run counter to predictions from other theories—then one may fall) and evidence that I can see, feel, hear, experience, and measure to test those predictions. I admire the work toward theoretical development by both of these scholars. But until those theories are fully developed and tested to my satisfaction, I do not intend to do anything with them.

Conclusion

We began with Quintilian and we may as well end with him. He recommended that what we repeat in the peroration ought to be repeated as briefly as possible, else it becomes a sort of second message. Accordingly, I have maintained:

1. Our problems with research evidence are more practical than they are philosophical.
2. Different standards for evidence pertain to the three different phases in scientific inquiry.
   a. The perspective in the first phase is expansive, searching, scanning. The findings from multiple complemenary approaches are sought. Standards for evidence are relatively relaxed. Tacit knowledge and intuition are admissible for hypothesis development. More speech communication research of this type is needed.

[15] Fiedler, pp. 83, 89, and 94.

b. Theory development occurs in the second phase. Research evidence is not a problem at this stage. Sound reasoning is the essential standard. Communication research has limited efforts in this phase and needs more.

c. Theory testing in phase three places rigorous demands on evidence. Evidence here must be unambiguous, public, and compelling. Although numerous published studies test very specific and narrow hypotheses, few can be considered tests of theory. Many are merely tests of limited and narrow questions.

3. We seldom see first level evidence of other researchers. We must often rely on the credibility of other researchers and the conformity of their assertions with our own experiences. There may be a need for a greater effort to obtain and evaluate first level evidence of others, increased replication of the studies of others, and more direct tests of second level evidence.

Interpretation and Evidence

JESSE G. DELIA AND LAWRENCE GROSSBERG*

THE QUESTION of this symposium, that of evidence, is raised in a variety of fields besides the sciences, particularly in law, rhetoric, and philosophy. In each of these fields, the primary concern is under what conditions our beliefs can be justified by appeal to "facts" in such a way that a statement of our belief can be taken to be a reasonable, if not a true, assertion. The essential question needing reflective scrutiny, therefore, is "what is a fact?" Our basic claim will be that facts constitute abstractions out of some situation to support some statement.

This claim has been argued by numerous authors. For example, William James attacked the "abstractive fallacy" by which empiricism dissociates its "facts" from the "world of pure experience." It was his view, as it is ours, that human experience involves a continuous lived relationship in which there is a unity of person and world, of knower and known. There are no "pure facts" existing outside this lived experience. The move defining the abstract empiricism underlying many of our contemporary ideas about science and reality involves a process of the deconstruction of the concrete texture of everyday life within which the empiricists' facts are embedded and the reconstruction of the world as a "succession of discrete, atomistic, sensory qualities." In short, facts represent abstractions out of a world of lived experience in which facts do not exist as isolated, discrete qualities or events.

The view of facts as isolated, discrete, and separate from some scheme of reference by which they are abstracted from pure experience, however, appears to be required for the project of the natural sciences, namely, that of the mathematization of reality.[1] It is not clear immediately that such a move is either essential or justified a priori for the human sciences. And even with respect to the natural sciences a number of authors (e.g., Polanyi, Hanson, Kuhn, Toulmin, Feyerabend) have argued that underlying the collection of abstracted facts, there is an ongoing process of interpretation within the scientific community (in Polanyi's term a "logic of tacit knowing") which allows the scientist to recognize and correlate facts. This perception of an ongoing process of interpretation pointing toward the meaningfulness of experience has led to "a general rejection" of the logical empiricist's program with its radical separation of fact and meaning.[2] The view has come more and

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[1] Aron Gurvitsch, *Phenomenology and the Theory of Science*, ed. Lester E. Embree (Evanston: Northwestern Univ. Press, 1974).

[2] Frederick Suppe, "The Search for Philosophical Understanding of Scientific

more to be accepted that "all our knowledge of the world, in common-sense as well as in scientific thinking, involves constructs, i.e., a set of abstractions, generalizations, formalizations, idealizations specific to the respective level of thought organization."[3] This means, as Schutz puts it, that "strictly speaking, there are no such things as facts, pure and simple."[4] Consequently, the ground of objectivity is shifted from questions concerning the correspondence of descriptions and reality (that is, questions of general criteria of evidence) to questions of the historically developed standards of rationality operating within particular communities.

Thus it should be clear that if the human sciences are to examine reflexively their grounds, such traditional separations between knower/known and fact/interpretation must be called into question according to the test of our own experience. Such is the task of phenomenology, which attempts to describe the world of prephilosophical and prescientific understanding from which both science and philosophy derive their facts and meanings. This originative life-world is one within which "world facts," to use a term of John Wild, cannot be described in terms of the empiricist's idea of fact nor the idealist's notion of essence.[5] While an explication of the concept of the life-world is beyond the scope of the present discussion, it should be emphasized that it indexes for us the meaningfulness of human experience prior to its being abstracted, categorized, conceptualized, or explained. Where theoretical knowledge is abstract and dependent upon particular schemes of reference for representation in the life-world, fact, value, and meaning coexist within a complex, dense texture which is always occurring within an ongoing process of unabstracted interpretive understanding.

This reflexively discovered origin of the scientific world is not offered to replace the foundation the empiricist seeks for knowledge, nor for the certainty the idealist seeks. Rather, it provides us with an understanding of the process by which the search is carried out and the distance existing between the worlds described by scientists and the concrete world of everyday life. We are not arguing that the sciences of human existence should attempt to describe, explain, or account for this life-world. This task, if it belongs to anyone, belongs to the philosopher. It is the nature of this reality (our experienced life-world) that it can be studied in a variety of ways through a variety

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Theories," in *The Structure of Scientific Theories*, ed. Frederick Suppe (Urbana: Univ. of Illinois Press, 1974), p. 62.

[3] Alfred Schutz, *Collected Papers, Volume I: The Problems of Social Reality*, ed., Maurice Natanson (The Hague: Martinus Nijhoff, 1971), p. 228.

[4] Schutz, p. 5.

[5] John Wild, *Existence and the World of Freedom* (Englewood Cliffs: Prentice-Hall, 1963). It is important to realize that the life-world is not the same as the everyday world of common-sense, although they are intimately connected. As we have noted, the latter is already an abstraction from the life-world. We are grateful to Calvin O. Schrag for many of the ideas in this analysis.

of abstractive principles. The human sciences must abstract from the concrete texture of meaningful experience some aspects and subject them to the analytic and interpretive techniques constituting the methods of the social sciences.

Thus the question of the meaning of fact (or evidence) is built upon the more fundamental question of the structure of human existence, a question of philosophical anthropology. In understanding the nature of fact and evidence, we are led to questions concerning the nature of knowledge and of the being who possesses it. Within such a reflexive move, the grounds or sustaining source of our research may be rediscovered as the originative structure of our meaningfully interpreted world. All science, including the study of communication, is rooted within the life-world. It is only because human experience is meaningful prior to theoretical abstraction and explanation that such explanations can themselves be meaningful. Within the experience of the life-world (that is, within the reflexive grounding of research), the question of evidence makes no sense. With this grounding, however, we may arrive at a tentative understanding of the problem of facts. We must accept the need for theories which serve as principles of description, or more accurately, of abstraction. Any theory is an abstraction, including those (such as we shall propose) characterized as interpretive. It is only within a theoretical framework that we can inquire into the nature of facts. Facts are experiences given meaning by the theory (and its correlative interpretive scheme) for which they exist as not in need of further examination. That is, facts are interpretations that are not called into question. Of course, such a judgment is a social and not an individual one. It is justified by the shared system of interpretation constituting the world of the particular scientific community and the meaningfulness of the life-world. Within any such interpretive schema, precision is possible to the extent that it represents a real possibility vis-a-vis the life-world. At the same time, the human sciences must recognize the ambiguity of human life and experience which allows it to be abstracted and interpreted in a variety of ways. Hence, multiple perspectives are a requirement for the social sciences because only such a view respects the ambiguity of human experience.

This reflexive move to the grounds of the human sciences has led us to a life-world (a pretheoretically meaningful experienced world) from which scientific facts are descriptions justified by their own abstractive principles. Given such a conclusion, one may ask what, then, is our answer to the question occasioning this symposium? It is quite simple and obvious: evidence ought to be good. Of course, we might flesh this out a bit and say that it ought to be reasonable, relevant, shared, etc. Further elaboration requires the choice of an interpretive framework constituting the theoretical world we are examining. Any other "criteria"—quantitative, observable, interpretive, proces-

sual, systematic, reductionistic, and so on—are choices to be made within a particular framework by a community with a shared theoretical perspective.

This would seem to leave us with a relativistic conclusion since, if evidence is theory-dependent, it would seem impossible to use evidence to justify a choice between or among theories. Two conditions mitigate against this conclusion. First, theories may partially share abstractive principles so that one theory may argue against and disprove another. This, it would seem, is how the natural sciences have progressed.[6] Second, science is always an abstraction from reality, the originary reality of the life-world. Theories whose conclusions violate the structure of the life-world may be judged inadequate. Thus, one can argue that theoretical justification comes, not from the comparison between or among theories, but rather from the relationship between a theory and its origin within the life-world. The elaboration of the relationships between theoretical worlds and the life-world, while a vital question for a grounding of the social sciences, is a task remaining to be accomplished. It is, however, along these lines that we would argue for interpretive theories of communication. Within the present context, however, we can at best provide a bare outline of our perspective. In providing such an outline, it should be clear that we are going beyond a general discussion to one embodying our own commitments.

All views of communication (indeed, all theories within the human sciences) are necessarily, although almost always implicitly, grounded in a philosophical anthropology, that is, in some conception of what it means to be human. Such a conception forms the very ground of one's tacit assumptions about the nature of communication, the nature of facts and evidence, and appropriate research methods. We would argue for some understanding of human existence in which interpretation is central, because only such perspectives can account for the diversity of philosophical anthropologies (and their derivitive theories of communication and knowledge). That is to say, only some stance premised upon the assumption that humans are interpretive beings can account for itself as a perspective. There are, of course, a wide range of philosophical anthropologies premised on different understandings of persons as interpreters. Indeed, we have personal disagreements over which interpretive stance provides the most adequate grounding for the human sciences.[7]

While alternative interpretive approaches to communication will vary in

[6] See, for example, Dudley Shapere, *Galileo: A Philosophical Study* (Chicago: Univ. of Chicago Press, 1974).

[7] For example, contrast Lawrence Grossberg, "Conceptions of Language and the Study of Communication," mimeo, Department of Speech Communication, University of Illinois at Urbana-Champaign, 1976, and Jesse G. Delia, "Constructivism and the Study of Human Communication, *Quarterly Journal of Speech*, forthcoming. See also the text of Anthony Gidden's address to the American Sociological Association in *Phenomenological Sociology Newsletter*, Vol. 4, Nos. 2 (Feb., 1976) and 3 (May, 1976).

their central concepts and details, all accept that communication is an emergent, creative activity through which human social reality is constantly being recreated, affirmed, repaired, and changed. Within interpretive social theories, person are agents of action, not mere responders to events. Actors are capable of creative originative action, and consequently, communication is not completely bound by its past, but involves an emergent process in which social, that is to say intersubjective, reality is constituted.

Interpretive views of communication recognize this creative, emergent process of the social reconstruction of reality as involving an interplay of individual interpretive processes and socially and historically constituted processes and contexts. While the individual's approach to reality is defined within ongoing processes of interpretation, the everyday world with which he or she is confronted is already meaningful. Social reality is predefined, presented as an already-meaningful whole. Thus, the individual, in the process of socialization, does more than simply coordinate his or her idiosyncratic beliefs about the world with those of the larger socio-cultural group in which he or she finds himself/herself; one embraces an entire universe of shared meaning and accommodates to the reality of the life-world. As a consequence, one acquires a range of socially and historically constituted vehicles (a common language, shared cultural understandings, typical modes of interpreting expressions and experiences) for interpretation and communication. However, communication always involves individuals with their own personal interpretive perspectives and projects acting in concrete situations. Communication, therefore, is more than mere behavior exchange.

While socially defined behaviors (primarily linguistic utterances) are the essential vehicle of communication, behaviors are always ambiguous. Indeed, Rommetveit argues persuasively that the essential character of all communication is reflected in the actualization through interpretation of semantic potentialities within a contextually fabricated architecture of intersubjectivity binding the interlocutors together; patterns of communication can only be understood, he contends, within the architecture of intersubjectivity, and then only as complex and dense meaning-structures within the lived experience of the concrete events. In short, communication involves messages, and messages are always socially meaningful only within some contextually and historically created system of intersubjectively shared understandings. This is not to maintain that messages do not involve informational or formal structures, but that such structures represent further abstractions from their dense meanings as acts within the ongoing process of interpretation. Communication cannot be decontextualized without destroying its originary meaningfulness for the interlocutors.[8]

[8] Ragnar Rommetveit, *On Message Structure: A Framework for the Study of Language and Communication* (New York: Wiley, 1974).

The researcher who would study the process of communication through which shared meanings are constituted must recognize that the human social world is meaningful to the participants in it before he or she comes to it. The social sciences differ fundamentally from the natural sciences. The natural sciences deal with a world of objects, the social sciences with a world of meaningful acts. While the natural scientist studies a world devoid of intrinsic meaning, the social scientist studies a world already meaningful for the participants in it. Therefore, as Schutz explains, the social scientist investigates a world which "is not essentially structureless. It has a particular meaning and relevance structure for the human beings living, thinking, and acting therein. They have preselected and preinterpreted this world by a series of commonsense constructs of the reality of daily life, and it is these thought objects which determine their behavior, define the goal of their action, the means available for attaining them—in brief, which help them to find their bearings within their natural and sociocultural environment and to come to terms with it."[9]

Evidence in communication research ought to be considered best which most completely respects the pretheoretical meaningfulness of the human social world while necessarily abstracting from its concrete realities in seeking general understanding of the processes by which it is constituted. What we are saying is that there is a structure and density to the meaning of communicative events and processes as they are actualized in concrete situations in the life of real persons, persons who are acting, feeling, thinking individuals engaged in shaping their own understandings and utterances and those of their fellows within a reciprocal, ongoing process of interpretation.

The typical psychometric methods common to the variable analytic tradition in communication research pays no respect to the density of meaning present in the pre-structured social world. Indeed, most measurement techniques commonly used in communication research seek responses to such narrowly defined questions (typically answered on bipolar adjectives or agree-disagree scales) that they run the risk of imposing their own structure upon the social world of subjects in the very act of data collection. Such analytic techniques as factor analysis, multidimensional scaling, and Markov analysis are also laden with their own ordering principles. Research based on such techniques treads near making subjects the dummy for the researcher's ventriloquism.[10] Thus, to us the difficulty with much orthodox communication research which focuses upon testing technical hypotheses with data collection and analysis techniques laden with their own highly abstracted analytic mathematical assumptions is that such research reflects theories and assumptions so distanced from the meaningfulness of the social world as to

[9] Schutz, p. 6.
[10] Delia.

yield abstractions without grounding. As such, many contemporary theories seem either trivial or nonsensical to everyday social actors, and we constantly face the charge of countless practitioners that what they learned in psychology, sociology, and communication classes failed to help them either in understanding the people they work with or in aiding those people to understand themselves.

But how, one may reasonably ask, can the density of meaning in lived communicative experience be respected in the research process? This is, surely, the crucial, inescapable question to be asked of those who would argue for interpretive approaches in the study of communication. While answers ultimately must be worked out in the ongoing dialogue of scholarly discourse, we, nonetheless, will offer three general directives that may serve to further the present inquiry.

First, work with a systematically articulated interpretive theory of communication. That is, arrive at some understanding of what are the constituent processes of the event of communication and of their interrelations that is consistent with some interpretive understanding of persons, communication, and human social life. We realize this admonition is about as helpful as saying be smart and do good work, but it says more—a sound theoretic orientation must be the starting point. Without a theory defining for us the kind of data to collect, the methods to be used in appropriately securing that data, and so forth, the research enterprise becomes hopelessly undirected and its results without meaning and coherence.

While it would seem reasonable at this point that we give specific abstractive principles characterizing any interpretive theory, we are unable to do so in the present context because a wide range of alternative stances, each with their own commitments, coexist within the broad domain of an interpretive perspective. Numerous attempts at such a characterization have been made, but it is our belief that they are inadequate precisely because they have reduced the commitments of the broad perspective to those of a particular theoretical stance.[11] Thus any characteriaztion of the interpretvie approach in general must accommodate stances as divergent as the Chicago school of symbolic interactionism, George Kelly's cognitive theory of personality, Heinz Werner's organismic developmental perspective, Raymond Williams' Marxist cultural theory, Alfred Schutz' phenomenological social theory, Aaron Cicourel's ethnomethodology, and so forth.

[11] See, for example, Thomas P. Wilson, "Normative and Interpretive Paradigms in Sociology," in *Understanding Everyday Life*, ed. Jack D. Douglas (Chicago: Aldine, 1970), pp. 57-79; Maurice Natanson, "A Study in Philosophy and the Social Sciences," in *Philosophy of the Social Sciences*, ed. Maurice Natanson (New York: Random House, 1963); and Alan Dawe, "The Two Sociologies," in *Sociological Perspectives*, ed. Kenneth Thompson and Jeremy Tunstall (Baltimore: Penguin, 1971). For a critical overview of such attempts, see: Lawrence Grossberg, "Dialectical Hermeneutics and the Human Sciences," Diss., University of Illinois, 1976.

Second, collect data relevant to the constituent processes pointed to by the theoretical orientation in a form respecting the data's structured, meaningful character within everyday human life. People quite naturally and spontaneously are about the business of constituting personally and socially meaningful worlds. One need not impose analytic techniques with their own (almost always unexplicated) ordering principles to get people to represent their personal and social meaning-structures; those meanings are reflected in their every saying and doing. Concretely, this implies, as one possibility, that subjects should be asked to perform tasks allowing them to report or reveal their own subjective and/or socio-culturally shared meanings. If their subjective understandings of other persons are relevant in answering a particular research question, subjects' interpersonal impressions should be elicited via techniques permitting them to provide the content and structure of their perceptions. Most frequently, this will involve the use of written or interview free response formats. Similarly, to secure data relevant to the behavioral components of the process of communication, the researcher should have subjects engage in tasks that let them communicate—put them in groups with a problem to solve, have them play password or some other communication game, give them situations requiring that they construct persuasive messages, observe them in the enterprise of routine talk, and so on. Securing data in a form retaining the subjects' own structuring of their personal and social meanings will thus variously require letting them write open-ended answers to situations the researcher poses, collecting audio or video recordings of their talk, taking detailed field notes as a participant observer, and so forth.

The final and crucial move in this view of the research processes becomes that of further abstraction. All scientific study of the social world involves its deconstruction and reconstruction from a particular theoretic perspective. We are, of course, arguing for an interpretive stance in this endeavor because we believe such orientations remain most open to the originary character of the events of human communication and the fabric of social life created through them. As a consequence, we believe they offer the best foundation for constructing viable accounts of the structure and process of communication. We wish to make clear, however, that we are not advocating an orientation taking as its task describing the originary unity of the life-world. The very act of data collection is the first act of abstraction. What we have advocated is that the process of data collection be conducted in such a way as to preserve to the greatest extent possible the pretheoretical meaning-structures of the original events.

Given such data, there are a number of possibilities that present themselves to the researcher. The most common move, which we shall examine in a bit more detail, involves a second abstraction—that of categorization and

measurement. However, we do not wish to ignore the possibility of a "hermeneutic" or critical-interpretive social theory which would attempt to read the data as a text, revealing as its significance the structures of the life-world concealed by the original abstraction. Although examples of such work are currently rare, the writings of Raymond Williams, Paul Ricoeur, and Clifford Geertz exemplify such an approach.[12] Furthermore, theoretic abstractions defined by procedures such as interactionist and *verstehende* sociology or structural analysis might also provide insight into significant aspects of communicative practices. The former approaches are reflected in the symbolic interactionist analyses of Anselm Strauss, the historical sociological studies of Max Weber, the analyses of the interpretive practices underlying interaction of Cicourel and his colleagues, the participant-observational analyses of children's communication through the identification of critical instances of Matthew Speier, and the like.[13] Structuralist analysis of communicative phenomena are offered in such works as Barthes' analysis of "mythologies," Leymore's analysis of advertising, and Metz' treatment of film.[14]

Nevertheless, the most common and, to this point, fruitful line of research involves the second move of abstraction—the process of measurement. Given data collected in a form as faithful as possible to the life-world, i.e. in as open a form as possible, the task of measurement becomes that of providing further abstractive principles for explicitly categorizing aspects of the data. Some categorization systems may, of course, involve assumptions concerning the relationships between categories and, hence, may involve classification in terms consistent with mathematization. One great task facing the development of interpretive approaches to communication resides in the identification of abstractive principles sufficiently descriptive of the character of the meaning-structures of human experience that they will reveal important consistencies and regularities in communication processes when concretized into systems of content and structural analysis.

Not all categorization systems for coding interaction or free response data should be considered as consistent with an interpretive approach. In fact, most interaction analysis research, premised on the assumption that

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[12] For example, see: Raymond Williams, *The Long Revolution* (Middlesex: Penguin, 1965); Paul Ricoeur, "The Model of the Text: Meaningful Action Considered as a Text," *Social Research*, 38 (1971), 529-62; and Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973).

[13] Each of these authors' approaches may be seen in the following illustrative works: Barney G. Glaser and Anselm L. Strauss, *Time for Dying* (Chicago: Aldine, 1968); Max Weber, *The Protestant Ethic and the Spirit of Capitalism*, trans. Talcott Parsons (New York: Charles Scribner's Sons, 1958); Aaron V. Cicourel, *Theory and Method in a Study of Argentine Fertility* (New York: Wiley, 1974); Mathew Speier, *How to Observe Face-Face Communication* (Pacific Palisades: Goodyear, 1973).

[14] See, for example, Roland Barthes, *Mythologies*, trans. Annette Lavers (New York: Hill and Wang, 1972); Varda Langholz Leymore, *Hidden Myth* (London: Heinemann, 1975); and Christian Metz, *Film Language*, trans. Michael Taylor (New York: Oxford, 1974).

communication is a process of behavior exchange, has in no way attempted to formulate abstractive principles consistent with an interpretive understanding of communication. Coding systems consistent with interpretive views of communication will provide ways of concretizing the meanings of acts, the processes underlying the interpretation of acts, and/or the interrelation between behavioral acts and interpretive practices.

The task of fashioning such analytic systems largely remains to be accomplished. However, we can exemplify our views by reference to the kinds of content and structural analysis systems presented in some of our own work. All the systems we will mention reflect a concern with individual competencies involved in social perception, the formulation of communicative strategies, or the interrelations among them. To abstract individual competencies out of the processes involved in creating and communicating meaning is self-consciously an attempt at only a partial understanding of communicative experiences. But understanding individual competency seems to be a significant aspect of communication and, therefore, to justify its abstraction.

The coding systems to be noted further abstract from social experience through a cognitive and developmental understanding of competency. That is, a theoretical perspective is employed taking competency to reflect underlying cognitive processes which develop according to a system of principles specifying change as hierarchical. When applied to the interpretive processes involved in the formation of interpersonal impressions, these hierarchical principles resulted in the analysis of cognitive complexity in terms of the structural increase in the number of elements reflected in the impressions.[15] When applied to the problem of representation and resolution of inconsistency in impressions, the principles have been concretized into a hierarchical system reflecting the extent to which inconsistency is either ignored, focused upon, or conceptually overcome through the use of integrating constructs.[16] When applied to the understanding of another's perspective, the principles led to a hierarchical system reflecting progressively the suspension of evaluation, the representation of one's understanding of the other's perspective, and the understanding of the other's understanding of one's own perspective.[17] More

[15] See: Walter H. Crockett, "Cognitive Complexity and Impression Formation," in *Progress in Experimental Personality Research*, II, ed. Brendan A. Maher (New York: Academic Press, 1965), 47-90.

[16] This hierarchical system of impression integration has been used in a number of studies by Crockett and his coworkers. Alternative versions of the system for analyzing naturally formed impressions and for those based on experimenter-provided inconsistent information are summarized, respectively, in Jesse G. Delia, Ruth Anne Clark, and David E. Switzer, "Cognitive Complexity and Impression Formation in Informal Social Interaction," *Speech Monographs*, 41 (1974), 299-308, and Jesse G. Delia, "Dialects and the Effects of Stereotypes on Interpersonal Attraction and Cognitive Processes in Impression Formation," *Quarterly Journal of Speech*, 58 (1972), 285-97.

[17] Claudia L. Hale and Jesse G. Delia, "Cognitive Complexity and Social Perspective-Taking," *Communication Monographs*, 43 (1976), 195-203.

specific systems applied to the analysis of narrower questions have also been created.[18]

The work of Clark and Delia applies a similar cognitive-developmental perspective to the development of listener-adapted communication. Their orientation places stress upon the interrelation of the individual competencies underlying the cognitive assessment of listener characteristics and the development of behavioral control over communication at the strategic level. They develop a series of coding systems for dealing with freely provided impressions of listeners and freely constructed persuasive messages. Drawing upon cognitive-developmental theory, they cast the categories of the systems in hierarchical form. In one study, persuasive strategies were classified according to the level of understanding of the listener's perspective implied in the strategy itself.[19] In a different study, impressions of listeners and messages were collected and social perceptions and behavioral control over communication were then interrelated within a hierarchical framework involving a whole series of general and specific coding systems.[20]

The ultimate role of interpretive approaches in our field, of course, will turn on their yielding analytic tools capable of revealing important regularities in the processes and practices of communication. The success of our own varied beginnings, however, convinces us that the development of such techniques may well make us forget the great value we saw in other, seemingly more "scientific" techniques, because of the more thorough grounding of such a reflective empiricism in the lived experience of the human social world and the consequent ability of such approaches to more fully capture the complexity of communication in human life.

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[18] Other systems were employed, for example, in the research reported in Delia, Clark, and Switzer; Allan N. Press, Walter H. Crockett, and Jesse G. Delia, "The Effect of Cognitive Complexity and of the Perceiver's Set Upon the Organization of Impressions," *Journal of Personality and Social Psychology*, 32 (1975), 865-72, and Jesse G. Delia, "Change of Meaning Processes in Impression Formation," *Communication Monographs*, 43 (1976), 142-57.

[19] Ruth Anne Clark and Jesse G. Delia, "The Development of Functional Persuasive Skills in Childhood and Early Adolescence," *Child Development*, in press; and Ruth Anne Clark and Jesse G. Delia, "Cognitive Complexity, Social Perspective-Taking, and Functional Persuasive Skills in Second-to Ninth-Grade Children," *Human Communication Research*, in press.

[20] Jesse G. Delia and Ruth Anne Clark, "Cognitive Complexity, Social Perception, and the Development of Listener-Adapted Communication in Six-, Eight-, Ten-, and Twelve-Year-Old Boys," *Communication Monographs*, in press.

In Retrospect

B. Aubrey Fisher

MORE OFTEN THAN NOT, symposia tend to raise more questions than they answer. This symposium seems to be no exception. Perhaps not so amazingly the original papers reflect distinct similarities in fundamental assumptions. On the other hand, philosophical differences are also discernible. This retrospective analysis attempts to trace these commonalities and differences as well as to engage in some "if-I-had-it-to-do-over-again" introspection.

The three papers proceed from quite different orientations: Delia-Grossberg from "philosophical anthropology," Scheidel from a theory-research relationship, and Fisher from a state-of-the-art analysis of communication. Nevertheless, the strains of fundamental agreement are evident. All three papers are clear in their insistence that one's theoretical orientation inevitably guides the selection and admissibility of research evidence. It is also encouraging to note that while communication scholars were once concerned with petty squabbles over what-is-communication issues, the field has apparently outgrown such quibbling and can now deal with and search for more valuable issues related to theoretical orientations and rationales. The other side of the coin is that we may have merely replaced petty definitional squabbles with petty theoretical squabbles. I hope not.

The initial papers of the participants appear to have clearly broadened the question from admissibility of evidence to the nature of theory and the relationship of theory and research. Interestingly enough, while all participants emphasize the nature of theory, only one of us saw fit to provide a definition of theory. Allport's definition, cited by Scheidel, is as good as any, so long as we do not quibble about this definition of theory as we formerly quibbled over the definitions of communication. That is, for example, predictive capability is a typical characteristic of theory, but it should not be considered an absolute criterion for either validating or invalidating a theory.[1] Then, too, the logical structure of a theory in some form of mathematics (typically predicate calculus) is quite common, but it is not even typical of many theories. Toulmin, for example, illustrates the variety of structures for formalizing scientific theory, observing that some natural sciences are "taxonomic" rather than axiomatic while others employ "graphical, rather than mathematical, techniques."[2] He goes on to note that the logical positivists

[1] "Hilary Putnam's 'Scientific Explanation,'" in *The Structure of Scientific Theories*, ed., Frederick Suppe (Urbana: Univ of Illinois Press, 1974), pp. 425-27.

[2] Stephen E. Toulmin, "The Structure of Scientific Theories," in Suppe, pp. 604-13.

have left a legacy in science philosophy which virtually mandates the expression of scientific theory in some form of predicate calculus:

> The crucial defect of the traditional approach on this view lies in its equation of the 'rational' with the 'logical.' By declining to admit into philosophical discussion any intellectual relation that is not amenable to formal analysis, the Viennese empiricists eliminated from philosophy the whole subject of 'conceptual change'—the whole question of how conceptual problems of a nonformal kind arise and are dealt with rationally within scientific enterprises. Having focused on the nature of 'logical systems,' the Viennese tradition in the philosophy of science thus ended by giving a misleading picture of the intellectual content of a natural science. . . .[3]

It is primarily for this reason, the idiosyncracy of theoretical structures, that I chose to view the theoretical perspectives in human communication from a nonstructural point of view. The "form" in which the theory is expressed seems much less important than "the *rational procedures* by which conceptual problems are dealt with in a developing science." A theory is, above all, an abstraction of reality and never a reflection of reality. Data themselves are not simple observations of reality; they are inherently interpreted abstractions from reality. It is the abstractional quality of theory—any theory—which must be consistently kept in mind when employing any perspective.

One other aspect of the broadened symposium question concerns the relationship between theory and research. Scheidel clearly points out the interdependence of the three phases or processes in applying theory/doing research. I agree wholeheartedly with Scheidel's analysis and maintain my original proposition that communication research is typically atheoretical, because these three processes are not operating interdependently in the communication research that is reflected in our journals and convention papers. Perhaps our orientation to variables and empiricism at the sacrifice of an awareness of theory stems from our too typical view of theory and research as synonymous. Of course, they are not. Any given research technique is atheoretical to the extent that it is content-free. Once a technique is applied to content, however, that method reflects some theoretical viewpoint of which the investigator is too often unaware. Merton is explicit on this point:

> There is, in short, a clear and decisive difference between *knowing how to test* a battery of hypotheses and *knowing the theory* from which to derive hypotheses to be tested. . . . However, it should be noted not only that instruments and procedures used in sociological (or other scientific) inquiry must meet methodological criteria but that they also logically presuppose substantive theories. As Pierre Duhem observed in this connection, the instrument as well as the experimen-

[3] Toulmin, p. 611.

tal results obtained in science are shot through with specific assumptions and theories of a substantive order.[4]

One general example of this interdependence of theory and research is the experimental technique which implicitly contains the logical form of the conditional—that is, *if* (antecedent condition), *then* (consequent result)—which is unique to axiomatically formalized theory. That theory and research are interdependent is a truism. That communication researchers typically recognize and take advantage of this interdependence is not clearly evident.

The differences among our initial papers also reflect differences, often subtle but nonetheless distinctive. Scheidel and Fisher characterize the field of communication as multiparadigmatic. Delia-Grossberg may be agreeing, but they would evidently prefer that it would not be. In short, Scheidel and Fisher do not explicitly impose upon the reader any advocacy of a particular theoretical orientation. Delia-Grossberg do —a perspective which is consistent with what my first paper characterized as an interactional perspective. (I will return to this issue later.)

Reading the Delia-Grossberg paper, one gets the vague and uneasy feeling that they implicitly assume the existence of some "right" or "correct" theory. This is admittedly an inference and may grossly overstate and distort their intent. Nevertheless, the inference embodies an issue which is patently false. Suppe baldly states that any choice between theoretical orientations can be made only "on some basis other than factual adequacy."[5] And Kaplan agrees most vehemently: "Truth itself is plainly useless as a criterion for the acceptability of a theory."[6]

In point of fact, the worth of a theory is ultimately dependent upon its acceptance by members of the scientific community. Scheidel suggests that this acceptance is a function of the credibility or expertise of the advocate-scientist. Perhaps. But certainly a scientific community evaluates a theory on the basis of its popular usage. We learn from and train students in using the models reflected in past research, practicing the duplication of these models. And we perpetuate these "exemplars" in our courses in communication theory and research methodology.[7] We thus become less adaptable to "conceptual change," which Toulmin considers is essential for a developing science. We may indeed be guilty of preparing soldiers to fight the last war.

Without quibbling over whether the four perspectives I discussed earlier are worthy of the term "theory," I have characterized them as *competing*—in the sense of both competing for popularity and as competing models of

[4] Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Illinois: Free Press, 1957).

[5] Suppe, p. 215.

[6] Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), p. 312.

[7] For a discussion of "exemplars," see: Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: Univ. of Chicago Press, 1970), and "Second Thoughts on Paradigms," in Suppe, pp. 459-482.

scientific explanation. Attempting to compare one against the other directly as in a "crucial experiment" has been a dream of many philosophers. But such direct comparison is apparently quite impossible. Different theories are simply "incommensurable" in that the data generated by one theory are different from and incomparable with data generated by another.[8]

But if theories are not capable of being compared via research, then how can one theory be evaluated in comparison with another? Maybe we should choose the perspective with superior explanatory power. But each perspective that has generated a sizable body of research has considerable explanatory power, even though the nature of the explanation is quite disparate from another theory's explanation. In one sense we choose to use and advocate theoretical perspectives reflecting a process not unlike that of deciding which brand of fluoride toothpaste to purchase. Perhaps the expertise or reputation of an advocate is a reason for buying into one theoretical perspective. Perhaps advocacy is attributable to one's limited exposure to a variety of theories to the extent that the student is trained in only one perspective and is consequently led to believe in that perspective as the only or the best one. In short, research evidence is "good" to the extent that we agree that it is "good." If this depiction of the state of affairs does not sound very "scientific," then so be it.

I would return at this point to the advocacy exhibited in the Delia-Grossberg paper. The substantive portion of this essay concerns itself with advocating the interactional-phenomenological-ethnomethodological position which characterizes communication as "lived experiences" (ala Schutz). I am tempted to ask where the evidence is for this theory, but that may not only be a trivial question but also probably inappropriate as well. The authors present this orientation as an answer to the question, "How does the investigator validate the data generated by research?" That is, where does one look in order to discover the "real" meaning of communication data? Answer —in the subjects' heads. The researchers suspend judgment, interpretation, and even classification, using focused interviews (for example) to allow the social actor to interpret "the density of meaning in lived communicative experience." This perspective is certainly valid and appropriate as one ap-

[8] For discussions of the "incommensurability" of theories, see, for example, Paul K. Feyerabend, "Problems of Empiricism" in *Beyond the Edge of Certainty*, ed., Robert G. Colodny (Englewood Cliffs, N.J.: Prentice-Hall, 1965), pp. 145-260; and "Consolations for the Specialist," in *Criticism and the Growth of Knowledge*, eds., Imre Lakatos and Alan Musgrave (Cambridge: Cambridge Univ. Press, 1970), pp. 197-230; and David Bohm, "Science as Perception-Communication," in Suppe, pp. 375ff. For a discussion of the "crucial experiment," see, for example, Feyerabend, "How to Be a Good Empiricist —A Plea For Tolerance in Matters Epistemological," in *Philosophy of Science, The Delaware Seminar*, Vol. I., ed., B. Baumrin (New York: Wiley, 1963), pp. 3-40; and J. J. C. Smart, *Between Science and Philosophy* (New York: Random House, 1968), pp. 81-87.

proach to guide communication research. But it is certainly not *the* perspective for communication inquiry.

One might even be tempted to argue against this perspective. One might argue, first, that the social actor is a biased source precisely because he/she did live those experiences. The observer is more objective and impartial. As a result, the investigator trades the problems of researcher bias for problems of subject bias. Second, the social actor does not have a vocabulary with which to express those personal experiences in generalizable communicative terms or concepts. The trained communication observer, utilizing a view gained from having observed many communicative acts and situations possesses the vocabulary and the classifications necessary to allow for generalizability of the data. Third, if the observer is the ventriloquist and the subject the dummy, then the converse is that the observer is the dummy to the subjects' ventriloquism. In fact, I am reminded of Scheidel's pigeons analogy in which the pigeons control and explain the meaning of the research data.

But do any of these arguments establish that the Schutzian perspective is inappropriate or of questionable validity for guiding communication inquiry? Certainly not! How can anyone argue that one perspective is intrinsically more "true" or "correct" than another? In some cases, usefulness may be an effective criterion for judging a perspective. And as a general theoretical perspective of communication at different levels of analysis, the Schutzian perspective becomes less useful. As Berger and Luckman point out: "The typifications of social interaction become progressively anonymous the farther away they are from the face-to-face situation."[9] While subjects' lived communicative experiences may be applicable to a mass or societal level of human communication, the problems of assessing past communicative experiences and associations of large bodies of people become increasingly more serious as one moves upward in the sociological levels from interpersonal to group to organization to society.

The most important issue in attempting to advocate one perspective over another is the relationship between observation, interpretation, and understanding. Clearly observation and interpretation are the same process—inseparable. Remember that the act of collecting data is inherently interpretative. But understanding is distinct. To derive meaning from an experience or an observation is not necessarily to understand it, to be able to make sense out of it. Ambiguous results in a research study are easily interpretable but not easily understandable; that is, why they occurred, what they mean, how they relate to other studies. The theoretical perspective allows for the merger of understanding with interpreting/observing data.

With the improved vision resulting from hindsight, one is able to discern

[9] Peter L. Berger and Thomas Luckmann, *The Social Construction of Reality: A Treatise in the Sociology of Knowledge* (Garden City, N.Y.: Doubleday, 1966), p. 31.

weaknesses not apparent at first writing. For example, not one of the papers sought to address the question of what renders research evidence *un*acceptable. As one of my colleagues so aptly phrases it, unacceptable evidence in research is a fact of life, so how do you deal with it? That is, when you cannot make sense of (i.e., understand) the data, then it must be unacceptable regardless of the form of the data or the method or perspective used in generation. What do you do when the research evidence provides many results but little explanation? Perhaps the answer lies in what we are trying to do with research. Are we attempting to support a theory (typically a "pet" theory)? Or are we trying to eliminate those theories which are less explanatory? In all likelihood we typically do the former and ignore the latter. We should be doing both.

Perhaps the greatest weakness of the earlier papers (principally my own) was the failure to question the theoretical perspectives. On the other hand, to engage in serious questioning of perspectives may be doomed to failure. Delia and Grossberg would be among the last to question Schutz. I would be among the last to question my own perspective. Scheidel may be the only open-minded person among us. Simply speaking, we tend to be ego-involved and defensive about our own theoretical orientations. We shall probably continue to advocate and wait for today's bright young graduates to make the conceptual changes in the next generation.

While all the participants mention the role of biases and preferences among investigators, we all skirt the issue somewhat. My view of the communication scientist is that of a competent decision maker guided by hopefully rational choice, arguing for the stance taken in the research and for the perspective chosen to guide the research. The scientist argues for the admissibility of evidence in the research. Consequently, the process of performing and especially reporting research is an exercise in rational argument.

When I initially characterized communication research as atheoretical, one of the problems I perceived was that the investigator allows too many of these decisions to be made for him/her. The excitement of the inquiry is not as evident as the quest for empirical results. Our research seems to be "designed to find out what is already believed to be true" rather than "designed to find out what was not even guessed at before."[11] Someone once suggested that all tasks can be reduced to just three—emptying things that are full, filling things that are empty, and scratching where it itches. The first two are things that "need" to be done. About those we have little choice. We verify or we falsify. But the third type of task is true inquiry based

[10] Norwood Russell Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridg eUniv. Press, 1958).

[11] Seymour Martin Lipset, Martin Trow, and James Coleman, "Statistical Problems," in *The Significance Test Controversy*, eds., Denton E. Morrison and Ramon E. Henkel (Chicago: Aldine, 1970), p. 84.

solely on the excitement generated from intellectual curiosity. Such research is, in the long run, infinitely more valuable.

Gerry Miller recently warned against "insistence on ambiguous, poorly defined 'oneness' or interminable quibbles over which methodological cookie-cutters to employ" in communication inquiry.[12] The field of communication is by no means unified; nor are we liable to cease arguing in hallways and on convention rostrums over methodological trivialities. This symposium will undoubtedly solve no issues; nor will it be long remembered. But at least we did not regress to the in-house squabbles of past decades. At worst, we have replaced the old squabbles with newer and more interesting ones. And that, at the very least, is some improvement.

[12] Gerald R. Miller, "Miller Replies," *Western Speech Communication*, 40 (1976), 283.

Afterthoughts

THOMAS M. SCHEIDEL

IT WAS THE INTENTION of this symposium to highlight important concerns and to stimulate thinking about the prerequisites for theoretical development in speech communication research. After reading and rereading the papers, it seems apparent to me that we symposium participants are largely in agreement that our discipline is not now able to employ fully developed theories of the type I described as central to Phase III of the scientific process. Rather, our concerns can be seen as the definition of essential concepts and variables, basic measurement, and the development and testing of hypotheses involving simpler relationships among communication concepts. Our present efforts, then, are occurring primarily and properly at the level of Phase I and Phase II. We seem to agree further on the necessity for research questions in Phase I which derive from sound conceptual (theory building) reasoning. In our own ways we each suggest that the tests of evidence cannot be absolute but must be relative to the particular research question and to the level of the particular research question asked.

The first thoughts occurring to me after reading these papers had to do with ways for improving our work. If one accepts the general conclusions of the symposium, then where should we go from here? What might we do better? What could we be doing that we are not? I am prompted to make two recommendations.

One of the clearest needs in communication research, as mentioned above, is the need for greater conceptual richness in theory building. Our conceptualizing often seems to be overly simplified and two dimensional. This shallowness is apparent often when we look at the findings of research studies. But how can the situations be improved? Multi-dimensional techniques are the current vogue and should be helpful to this end. I believe another answer may be found in the attempt to bring a greater depth and range of resources and references to our conceptual efforts. It would seem to me that *a significant improvement in conceptual richness could result from a more historical perspective in our scientific research.* Let it be clear that I am not asking here for the perspective of an historian of science, but rather for the view of a scientist with an historical perspective.

Published studies often carry the implicit view that serious research on the topic, whatever it is, had its origin in very recent years; perhaps even with the present researcher. This self-conscious and self-centered aspect of the scientific enterprise, lamentable but natural, is partially what I was point-

ing to in the early remarks in my essay. I hesitate somewhat to sing the song of tradition but I am offended when an apologist for interpersonal communication portrays the "traditional" public speaking teacher as sterile, static, and concerned only with "telling" others and "manipulating" others. James A. Winans, Herbert A. Wilchelns, and Karl Wallace were not simple men. If someone is to speak for them, the words should at least be correct.

Alan Monroe's dissertation, completed nearly half a century ago, was a factor analytic, multi-dimensional investigation. He conducted this work without the aid of IBM cards and computers and, I would guess, gained some intimate appreciation of his data and procedures since he had to have spent so much time with them. It should be useful for speech communication researchers to know about this heritage. When I was editing *Speech Monographs*, we received several papers detailing *new* methodological advances — some of which we found had been employed in articles published in *Speech Monographs* as much as 30 years earlier. Ours is an old discipline. We have a heritage and we should know it, and *use it*. The Romans did not have telephones and television, but they did have speech, language, interpersonal relationships, small groups, public forums, arguments, ceremonies, readings. They experienced speech communication; they thought about it, and they wrote about it. While some contemporary concepts of speech communication may take newer directions, none can fail to have historical ties.

I do not understand, as an example, why Irving Janis wrote about Groupthink and hypothesized about its internal causes without any mention of all the directly related conceptualizing about Group Mind and its internal causes. Janis must know all the background, but many of his readers will not. I believe it would have been helpful if this research scientist had de-emphasized the self and the implicit argument from authority and had set his work in a fuller perspective. That would have provided more information (evidence) for the reader of his work, and would have facilitated the readers' efforts at evaluation.

There is an additional reason for the historical perspective. It has been noted that the five canons of rhetoric, with us always, have received different emphases in different societies at different times. It can be argued that as a communication concept *reemerges* and plays a relatively more significant role, it reflects something of the age and the society that has made it so salient. And these special factors would likely also influence research on that topic. Nonverbal communication is an ancient concern which has reemerged or been rediscovered. It is now a popular topic for research and for course offerings. It would be an interesting research question in itself to ask how it is that this topic should reemerge at this particular time. How is this topic treated differently now from before? What may all of this reveal beyond the concept itself about our society and our age? None of these questions would

arise without the historical perspective.

As a second recommendation let me urge, since we agree that tests of evidence are relative to the particular research question and investigation, that *scientific researchers should take on more of a rhetorical burden for establishing the claims of their evidence.*

In the typical data-based research article, there is a large unit devoted to introduction, background, and defense of the topic. The reader is persuaded that the topic is important. Another large unit presents the discussion of results, implications, and suggestions for future directions. These beginning and concluding sections usually contain arguments that the study was worth doing and produced something of value. The arguments in these sections are explicit; reasons and evidence are adduced; the inferences are fleshed out and supported. Then, there are the other sections; a brief overview of method and procedures, and a somewhat more detailed presentation of results, supported with tables and figures. These sections, differing from introduction and conclusion, tend to be focused exposition. They describe and explain but do not argue and support. The stance is informative and not persuasive.

If the position of this symposium about the relativity of research evidence is correct, then I would suggest that more direct supporting argument would be appropriate in the procedures and results sections of research reports. The researcher could inform us not only about what procedures were used, but about why they were chosen. With what were they compared? Why were these procedures the best possible? What assumptions were violated? What shortcuts were taken? A specific approach to data analysis may (1) not be inappropriate, without (2) being the most appropriate. Researchers usually inform us of (1) but rarely persuade us of (2). It would be interesting to see what effect the placing of this added burden upon a researcher might have for our research reports. Any extra publishing space required could easily be obtained by cutting the frequent lamentations about what would have happened if the study had been done differently, as it might be some day.

If, after reading all the positions in this symposium on evidence in scientific research, the reader should react "Well, I don't agree," it would not be surprising to me. I believe the self plays a more significant role in the process of scientific inquiry than we usually acknowledge. As Alexander Pope wrote: "Tis with our judgments as our watches; no two go just alike, yet each believes his own."

In Response

LAWRENCE GROSSBERG AND JESSE G. DELIA

SIGNIFICANT SIMILARITIES and important differences exist among the three contributions to this symposium. These similarities and differences need to be understood in a critical way in order that the issues emerging from the juxtaposition of the papers be clearly recognized. Accordingly, we shall isolate a number of these issues to make them available for further critical public scrutiny. In particular, we shall focus on three broad areas: the role of philosophy in the research enterprise, the nature of the scientific process, and the question of evidence in communication research.

Professor Scheidel explicitly raises the most fundamental question underlying the idea of the symposium, namely, whether questions at the level of philosophical inquiry have relevance to the conduct of the study of communication. His citing of Abraham Kaplan to the effect that philosophical findings have no bearing upon the research of the social sciences constitutes the most fundamental challenge to our own views on the relationship between philosophy and communication inquiry. However, although Professor Scheidel seems to deny to philosophy an essential role, it is obvious that his own reflections on science and evidence are a reiteration of the logical empiricist description of science (toned down by a Popperian falsificationism), i.e., a philosophy in its own right. The question, it would seem, is not whether philosophy has a role, since Professor Scheidel relies upon one, but rather, what that role is and how it should be actualized.

This issue can best be clarified by considering the divergent views expressed within the papers of Scheidel and Fisher. While the two appear to have quite different conceptions of the nature of science, theory, and data, neither addresses the problems raised by their respective analyses. The two papers cogently express the two dominant orientations to the scientific enterprise commonly held within the community of communications scholars. Scheidel, operating within the traditional, logical empiricist view, sees the observational-descriptive phase of science as distinct from the theoretical-explanatory phase. Such a view necessarily assumes an independence of theory and observation, a view which, as we have noted, appears to be no longer defensible. Fisher, on the other hand, operates with a Kuhnian view of the interdependence of theory and observation. Within his analysis, this view appears to lead to a gross relativism leaving us with no basis for choosing among competing perspectives; consequently, our knowledge is left fragmented, and we appear to have little or no hope of understanding the unity of our discipline.

While surely we have not solved the dilemmas lying within the gulf be-

tween these two commonly held stances, our analysis has pointed to the possibility of steering between the Scylla of abstracted empiricism and the Charybdis of relativism. This possibility exists within the recognition of the essentially reflective character of philosophical thought. It is, in our view, not sufficient for a philosophical position to describe the commonly held attitudes and beliefs characterizing a particular community. Instead, philosophy, at least at the present level of inquiry, involves a calling into question of the underlying assumptions (and their ramifications) of a particular domain of knowledge. Only when such assumptions are critically examined and the question of their own grounding raised in the light of a reflective description and interpretation of human existence that the dilemmas of abstracted empiricism and relativism can be avoided. We agree with Natanson when he says that "philosophy is inescapable for the social scientist who seeks clarity and rigor in his work, who takes the term 'discipline' seriously."[1]

As the foregoing discussion implies, there is a second set of issues raised by the three papers concerning the nature of scientific inquiry. The first issue turns on the role of theory in the research enterprise. While Scheidel seems to acknowledge grudgingly, as do the later logical empiricists such as Hempel, that theory plays an important role in all stages of research, he nevertheless attempts to relegate theory to a role involving largely the systematization and organization of knowledge within a functionally explanatory system. Fisher, by contrast, appears to see theory as much more central in the conduct of research. Yet, even he does not appear to acknowledge fully the pervasive presence of theory in research. This is evident for example in his characterization of research within the variable analytic tradition as "atheoretical." Such research can be referred to as atheoretical, however, only in the sense that it involves the naive acceptance by the researcher of unexplicated theoretical constructs and positions which have defined particular "variables" as relevant to understanding communicative processes and practices. Furthermore, variable analytic research also unreflectively accepts a host of methodological prescriptions defining a particular orientation to the "proper" conduct of research.

Fisher's understanding of the variable analytic tradition seems to result, in part, from his somewhat vague and problematic notion of a theory. His "epistemological classification" of perspectives in communication theory, while acknowledging the value and importance of theoretical differences within the ongoing development of a discipline, appears at best to be ad hoc and piecemeal. The identification of "perspective" and "theory" tends to ignore the often significant differences existing among alternative theories within a

[1] Maurice Natanson, "Alfred Schutz on Social Reality and Social Science," in *Phenomenology and Social Reality*, ed. Maurice Natanson (The Hague: Martinus Nijhoff, 1970), p. 101.

particular perspective. At the same time, each perspective is characterized only by some fairly simple and straightforward feature rather than by a complex nexus of assumptions and commitments.

As we have tried to show, theories must be seen as embedded within an ongoing process of interpretation grounded in the human life-world; this is true in scientific, as in all inquiry. Theoretical constructs, whether reflectively or naively held, constitute an essential set of the abstractive principles constituting the very essence of the research enterprise. It is only on the basis of a full and reflective appreciation of the nature and role of theories, which neither Scheidel nor Fisher succeeds in articulating, that the possibility exists for fruitful metatheoretical dialogue. Furthermore, while we agree with the sentiment behind Fisher's advocacy of multiple perspectivism, it is only in the context of such reflective philosophical grounding that multiple perspectivism can be embraced without the spectre of an irrational relativism which seems to deny the possibility of any evaluations among alternative theories.

This leads us to the second issue regarding the nature of the scientific endeavor. For both Fisher and Scheidel, the scientist seems to operate essentially in isolation. On the other hand, we have attempted to emphasize that the scientist works within a community. While there are inescapable personal commitments in the conduct of inquiry, the choices among competing theoretical paradigms are made within the context of larger community decisions. Thus, the evidence upon which such choices inevitable turn embody both the standards of rationality historically developed as part of the meaning of science in our culture and the particular tests and criteria of the specific scholarly community. It is this historically developing, shared background knowledge—a set of taken for granted assumptions shared by the members of the community—which partially provide the limits for multiple perspectivism within a particular discipline.

The final issue raised by these papers that we wish to address returns us to the question of the nature of data and evidence. It is clear to us that there are significant differences among the contributions concerning the locus of the issue itself. That is, if we understand the question of evidence in terms of a structural relation (for illustrative purposes only), we may see that each paper takes different terms as defining the relationship of "being evidence of." Scheidel discusses the question of evidence in the various phases of science by contrasting what he calls first and second level data. The former refers to the data as collected, while the latter refers to the data as analyzed and summarized in the research report. Although the nature of this first level data is never critically examined, Scheidel seems to locate the question of evidence in the relation between these two levels of data. Fisher, alternatively, places greatest emphasis on the question of the relationship between theoretic constructs and the domain of data defined as relevant to the inquiry itself (pre-

sumably by those constructs). While it seems that Fisher recognizes the theory-ladenness of this data, as we have said, his discussion of "atheoretical research" would seem to make this depiction of his position problematic.

Our own position attempted to articulate reflectively the grounds out of which both Fisher's and Scheidel's views of the problem arise. In so doing, we raised the question of the relationship between the abstractive principles constituting first level data collected within any investigation and the more originative notion of data existing within the context of the human life-world. Within this context, Scheidel's understanding of the problem of evidence can be treated as a second-order abstraction performed on the data. Fisher's location of the problem of evidence as well has been raised within our own view in terms of the necessity for abstractive principles within the research enterprise. Our view, however, attempts to go beyond these orthodox structures (as they exist within the unexplicated commonsense of the practicing researcher) to a reflectively grounded view which considers not only first and second level data (in Scheidel's terms) and the theory-ladenness of data, but also the experiential origin of evidence within the life-world and the ongoing historical and communal process of interpretive understanding.

Thus it is that we called for an orientation in communication research which may be characterized as "reflective empiricism" as opposed to abstracted empiricism which has lost its originary relationship to human existence. Our view requires that the communication researcher must eschew methods that yield first level data (in Scheidel's sense) in a highly abstracted and already quantified form. Such data seems to us typically to have import only in terms of some system of theoretic constructs whose relationship to the meaning structures of the everyday communicative experience of persons is at best highly problematic and at worst meaningless. The orientation to research we have called for mandates that the researcher work within a conceptual orientation built upon philosophical reflection acknowledging its grounding in the life-world; the researcher should work with "first level data" that is structured as little as possible in the process of its collection.

In our view, it is the connection between first level data and its ground or origin in human experience which defines the limits on multiple perspectivism and abstract empiricism. Within such limits, particular commitments to particular theoretical constructs (i.e., abstractive principles) must be made which guide the attempt to identify significant regularities in the communicative process. And it is within such theoretical choices that the specific criteria for evidence can be appropriately articulated. Apart from such contexts, all that can be said is (1) evidence ought not to occlude its relationship to the life-world, and (2) evidence ought to be good—that is, it must be compelling and convincing within the schemes of rationality employed within a particular scientific community.

Epilogue for Apologia: On the Convergent Validation of Epistemologies

Jo Liska and Gary Cronkhite

With the three position papers and the three rejoinders before us, the obvious question is what to make of all this. Our general reaction is that, if we collect the criteria from each paper, we have a list of criteria which are not too restrictive but still fairly comprehensive. In the rejoinders, however, there is some question raised as to whether the criteria advocated by Fisher and Scheidel, which seem fairly compatible, can be combined with those advocated by Delia and Grossberg.

We believe the primary problem is a confusion over the distinction between epistemological "theories" and explanatory "theories." All four writers use the word "theory," but no one says which type of theory he is dealing with. Fisher seems to use the term in both senses at different times, whereas Scheidel seems to use it only with reference to *explanatory* theories. Delia and Grossberg write about their "interpretive theory," which clearly seems to be of an epistemological nature, but they argue for the adequacy of that theory as if it were explanatory; that is, they try to *defend* what appears to be their basic assumptions.

To try to escape this confusion, we will try to do three things in this conclusion: (1) review the proposed criteria in light of the distinction between epistemological and explanatory theories; (2) identify insofar as possible the underlying assumptions or epistemological perspectives from which the writers seem to be operating; and (3) suggest an approach by which we may eventually resolve disagreements among epistemological theories, which approach may serve as a rationale for cooperative scientific endeavors based on a variety of epistemological metaphors.

Summary of the Criteria

Fisher and the Process of Theory Development

Fisher is proposing criteria which apply most clearly at the level of *explanatory theory development.* It also seems clear that he considers "doing science" to be a *process* designed primarily to *perform that function* of explan-

atory theory development. Thus his criteria are those which would best qualify evidence to serve that function. Unfortunately, when he complains about too much communication research being "atheoretical," and when he uses the term "theoretical development," he seems to be referring to explanatory theory. On the other hand, when he specifies that evidence ought to be "consistent with the conceptual theoretical rationale," it is not quite clear which type of theory he has in mind. If evidence is completely consistent with an explanatory theory (e.g., dissonance theory), it would not provide a test of the theory (e.g., subjects who did not act "dissonant" would be excluded). We suspect that Fisher's first criterion might be rephrased, "Evidence should be consistent with the *epistemological theory* on which is based the *explanatory theory* it is intended to support."

Fisher's second and third criteria seem to be relatively straightforward. We take them to mean that evidence ought to be collected in a way that enables the research question to be answered, and that evidence ought to include communication either as a manipulated or observed variable. Those admonitions may sound obvious, but one of the present writers has recently read two studies submitted for publication in which: (1) the research question asked about the relative persuasibility and credibility of women in comparison with men, but no men were included in the study; and (2) the research question dealt with the comparative utility of two measures of code restrictedness in two different contexts, but only one of the two contexts was included in the study. Further, one of the present writers, in critiquing the data-based papers appearing on Behavioral Science Interest Group programs at the Western Speech Communication convention in San Francisco last November, noted that only six of the sixteen studies included any communication beyond instruction to the subjects and only three of those manipulated or observed message variables.[1] Thus Fisher's second and third admonitions may not be as obvious as might at first appear.

In his fourth criterion, Fisher once again uses the terms "theory" and "theoretical development," this time apparently with reference to *explanatory* theory.

Scheidel and the Phases of Research

Scheidel proposes different criteria to apply to each of three phases of research he describes: (1) observation, analysis, and description; (2) development of theory; and (3) testing of specific predictions. Happily, Scheidel defines "theory" quite specifically, and it is clear that he is discussing *explanatory* theories. In fact, Scheidel specifically says he believes our problems are more practical than philosophical, and it is only with some difficulty

[1] Gary Cronkhite, "Some Discrepancies Between Verbalized Opinions and Observed Behavior of Behavioral Scientists," a paper presented at the Western Speech Communication Association Convention, San Francisco, 1976.

that one can even detect hints of the epistemological theory upon which he is operating. Thus we can read his criteria with considerable assurance that the term "theory" is being used unambiguously. In the observation phase he advocates "multiple complementary approaches" and approves tacit knowledge and intuition; in the theory-building phase, he holds that "sound reasoning is the essential standard;" but, in the theory-testing phase, he insists that evidence be unambiguous, public, and compelling.

Delia, Grossberg, and an Interpretive Stance

Delia and Grossberg use the term "theory" quite ambiguously, frequently to apply to explanations, but almost equally frequently to characterize their interpretive framework, which seems much more nearly a theory of how one knows the world (i.e., an epistemological theory) than it is an explanation of phenomena. Of course, they use the interpretive theory sometimes as an epistemology and sometimes as explanation.

Their criteria for evidence are: (1) evidence ought to be collected in such a way that it does not interfere with or impose constructions upon the interpretive processes of one's subjects; and (2) evidence ought to "be compelling and convincing within the schemes of rationality employed within a particular scientific community."

The second criterion contains no ambiguities with respect to use of the term "theory." Under the first criterion, however, are listed three subcriteria: (a) one ought to "work with a systematically articulated interpretive theory of communication;" (b) one ought to collect data so as to respect the data's structure; and (c) one ought to abstract by categorizing in ways "consistent with interpretive views of communication."

The problem here is this. If "interpretive theory" is *explanatory*, we certainly ought not to use it as a basis for collecting or categorizing data, since that will give it an unfair advantage when compared to competing theories. On the other hand, if "interpretive theory" is *epistemological*, then Delia and Grossberg, unlike Fisher and Scheidel, are arguing that researchers should adopt *one* epistemological approach. Indeed, reading their response to Fisher and Scheidel, it is apparent that is exactly what they are arguing. Thus one of the criteria for acceptable evidence, according to Delia and Grossberg, is that it be collected on the basis of the assumptions of phenomenology as modified by Schutz' philosophical anthropology. Two matters disturb us: (1) this is an extremely narrow definition of acceptable evidence; and (2) in fact, it is so narrow that the only examples of such evidence in the field of communication occur in studies they themselves have conducted.

On the other hand, if we were to be allowed to change one word in subcriteria (1a) and (1c), we would wholeheartedly favor adopting all the criteria Delia and Grossberg advocate. That is, if we were able to change the

word "interpretive" to "epistemological" in (1a) and change "interpretive" to "one's" in (1c), they would be perfectly acceptable, and would read: (1a) one ought to work with a systematically articulated [epistemological] theory of communication; and (1c) One ought to abstract by categorizing in ways consistent with [one's] views of communication. But would Delia and Grossberg buy it?

EPISTEMOLOGICAL PERSPECTIVES

Suppose we consider the epistemological perspectives offered by each author. Fisher maintains that the extent to which the evidence is consistent with the "conceptual/theoretical rationale" is the most important criterion for evaluating appropriateness. As we noted earlier, we assume that his use of "conceptual theoretical rationale" is analogous to "epistemological perspective."

Grossberg and Delia respond to Fisher's epistemological eclecticism by saying "this view appears to lead to a gross relativism leaving us with no basis for choosing among competing perspectives. . . ." At a later point in their response, they say:

> Furthermore, while we agree with the sentiment behind Fisher's advocacy of multiple perspectivism, it is only in the context of such reflective philosophical grounding that multiple perspectivism can be embraced without the spectre of an irrational relativism which seems to deny the possibility of any evaluations among alternative theories.

The germ of an idea in this epidemic of words appears to be that Fisher provides no criteria for choosing among competing epistemological perspectives. We, on the other hand, think Fisher's criterion of appropriateness provides a very clear criterion for choosing among competing epistemological perspectives.

Scheidel's attention is explicitly focused on explanatory rather than epistemological theories. However, Grossberg and Delia have concluded that Scheidel's epistemological perspective is best characterized by the term "logical empiricism." "Scheidel, operating within the traditional logical empiricist view sees the observational-descriptive phase of science as distinct from the theoretical-explanatory phase. Such a view necessarily assumes an independence of theory and observation, a view which, as we have noted, appears to be no longer defensible." Before we turn to Scheidel's paper for direct refutation of Grossberg and Delia's charge, we would like to turn our attention to a noted logical empiricist, A. J. Ayer, for his description of the verifiability principle, the cornerstone of logical empiricism:

> The criterion which we use to test the genuineness of apparent statements of fact is the criterion of verifiability. We say that a sentence is factually significant to any given person, if, and only if, he knows how

> to verify the proposition which it purports to express—that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false.[2]

We can find no evidence in Scheidel's paper that indicates he would adopt such a narrow stance as that articulated by Ayer. In fact, Scheidel argues that multiple complementary approaches are necessary and desirable in the first phase of research. Further, he acknowledges the importance of intuition and tacit knowledge in guiding the research process.

Let's return to Grossberg and Delia's comments. First, they claim that Scheidel sees the observational-descriptive phase as distinct from the theoretical-explanatory phase. Yet Scheidel states that "Scientific advance can be viewed as the result of three *interdependent* [italics ours] phases and processes." He goes on to state that "These three processes are circular and interactive." Further, he maintains "that scientific advance will best result from the combining of these three processes," and dwells on the areas of overlap among them. Unlike Grossberg and Delia, we conclude that Scheidel sees all three phases of research as necessarily interdependent.

In fact, Delia and Grossberg's concern with "concretization," measurement, and validation of categories might make them equally good candidates for the label "logical empiricist," if they had not elsewhere specifically denied it.

After careful examination of Scheidel's paper and the tenets of logical empiricism, we can find no evidence to establish that Scheidel's epistemological perspective is logical empiricism. Indeed we have some difficulty discerning which epistemological perspective Scheidel adopts. He does mention systems theory, which might constitute an epistemological perspective. He also suggests some pragmatic tenets ala Dewey. He does not, however, fully develop either as an epistemological perspective. What he does do is characterize the process of doing research as a problem-solving endeavor, and he does so in such a way as to remind us of the phases and process advocated by those in small group research which are adapted from Dewey's stages in problem-solving. Such a stance suggests that doing research is analogous to problem-solving, it is a practical endeavor, and the primary focus is on the specific problem at hand.

Unlike Fisher and Scheidel, Delia and Grossberg advocate the adoption of a *particular* epistemological perspective as *the* epistemological perspective. They recommend phenomenology or an approach derived from phenomenology, i.e., constructionism, interactionism, as the epistemological perspective. Phenomenological analysis is purely descriptive and "seeks to present as

[2] A. J. Ayer, "Verifiability and Truth," in *Perspectives in Philosophy*, ed. Robert N. Beck (New York: Holt, 1969), p. 283.

unbiased and 'pure' a description of experience as humanly possible."[3] Berger and Luckmann insist that "The phenomenological analysis of everyday life, or rather of the subjective experience of everyday life, refrains from any causal or genetic hypotheses, as well as from assertions about the ontological status of the phenomena analyzed."[4] The result of phenomenological "thinking" is a fuller insight into experiences and their meanings.

Such an epistemological perspective implies that a person is a decision-maker, an active agent who constructs his or her environment. As Kelly observes, a person "looks at his world through transparent patterns or templates which he creates and then attempts to fit over the realities of which the world is composed."[5] These templates or constructs are used to construe, perceive, interpret, and give meaning to objects, events, and other persons. Undeniably these constructs are, to some extent, idiosyncratic. Yet shared constructs will result from shared experience, experience that results from membership within common social and cultural communities.

As Royce puts it in *The Encapsulated Man*, "the essence of encapsulation is to offer *one* approach to reality as if it were *the* approach."[6] While Delia and Grossberg show definite signs of "encapsulation," the real problem is that they base their arguments on the inherent correctness or "truth" of their perspective, not on the results of research generated by their perspective. As Royce argues: "The adequacy of the assumptions in a belief system cannot be judged by the point of origin of the assumptions, but rather, in terms of the consequences."[7]

CRITERIA FOR EPISTEMOLOGICAL VALIDATION

We hold that the goal of scientific inquiry is to produce myths that increase the human specie's chances of survival and enhance an individual's ability to adapt to and cope with his/her environment. This goal requires the scientist to be sensitive to the problems and concerns of the society of which he/she is a member. Thus, science is a human activity concerned with the problems humans encounter and must cope with. Wartofsky writes:

> Science is neither a separable body of knowledge, abstracted from all intercourse with human life; nor is it simply what goes on in the thought processes of scientists or what gets codified in the textbook. Science is a human activity, a messy and involved way of getting along

[3] Adolf Reinach, "Concerning Phenomenology," in *Perspectives in Philosophy*, ed. Robert N. Beck (New York: Holt, 1969), p. 20.

[4] Peter L. Berger and Thomas Luckmann, *The Social Construction of Reality* (New York: Doubleday, 1967), p. 20.

[5] George Kelly, *The Psychology of Personal Constructs* (New York: Norton, 1955), pp. 8-9.

[6] Joseph R. Royce, *The Encapsulated Man* (New York: Van Nostrand Reinhold Company, 1964), p. 165.

[7] Royce, p. 18.

> in the world. It is also one of the most highly evolved adaptive instruments of our species life, and as such it bears the impress of its development, as a social and historical mode of cognitive practice.[8]

Scientific inquiry is a human enterprise. The basic research method is "the human intelligence trying to make sense out of what it observes." When "doing" science, the observer is inescapably a part of the phenomena he or she is studying and thus influences and is influenced by the phenomena. Wartofsky comments that:

> The scientist is the product of his society and of his socialization: He thinks, speaks and writes in language which itself marks him as being in communication with others; his scientific style, choice of problems, and of solutions to those problems are all hallmarks of his sociality, of his scientific community as well as of the larger community in which science is practiced.[9]

The norms of science, like those of any human activity, are reflections of changing social and historical realities. Therefore, the process as well as the products of science are subject to a variety of constraints, e.g., what constitutes truth, knowledge, and acceptable techniques.

> The epistemological constraints on science are social, and the social constraints on science are epistemological; and both our ways of knowing or our modes of scientific inquiry and the institutional forms and social uses of this knowledge are historical. Ways of knowing change with social practice and in turn, social practice and social structure changes with shifts, revolutions or transformations of our ways of knowing.[10]

Therefore, the process of generating scientific myths reflects the problems and concerns of the people who engage in doing science and the problems and concerns of the society to which they belong.

A scientist's arguments for a particular explanatory theory are based on an epistemological myth or theory of knowing, whether or not the scientist is aware of it. Typically a scientist is aware that he/she is arguing for a particular explanatory theory but does not consider that those arguments are based on an epistemological theory and by virtue of being so based constitute statements of faith in the epistemological theory as well.

The three papers in this symposium confound these two types of theories. Some of the arguments among the participants are the result of this confounding. Different explanatory theories may be derived from a single epistemological theory. In such a case, the methods appropriate to that epis-

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[8] M. W. Wartofsky, "Social and Epistemological Constraints on Science," a paper presented at the American Association for the Advancement of Science, 1974, p. 1.
[9] Wartofsky, p. 1.
[10] Wartofsky, p. 2.

temological theory can be used to decide among competing explanatory theories.

On the other hand, different explanatory theories derived from different epistemological myths cannot be so directly compared. In this conclusion we want to advocate a "problem-centered" approach to choosing both explanatory and epistemological theories. It is not unusual to find people who advocate choosing among *explanatory* theories depending upon the problem at hand. We are also advocating choosing among *epistemological* theories depending upon the problem at hand.

An example from research in language will illustrate our point. Descriptive linguists before Chomsky used an epistemology that required no dabbling in the thoughts and feelings of langauge users, simply because the problems they faced called for *descriptions of language*. In descriptive linguistics, a "theory" is a proposed description of a language. Two competing theories could be compared as to their adequacy without resorting to speculation regarding internal events. Chomsky, however, became interested in the extent to which the study of language could shed light on human cognitive processes, which has caused some psycholinguists to begin asking whether surface structure rules and transformational rules are merely descriptions of how sentences *might* be generated or whether they are descriptions of how language users *actually* produce and process sentences. That required a very different epistemology. But the new cognitive theories of how language users actually process language can hardly be compared to theories which were designed, using a different epistemology, to describe a given language. There is no basis for comparison. However, one of the questions of interest to cognitive theorists and linguists alike is whether there are certain aspects which are common to *all* languages—linguistic universals which might give a clue to the nature of human cognition. Here is a problem upon which both descriptive linguists and psycholinguists can concentrate, despite their epistemological differences.

Briefly, our point is this. When alternative explanatory theories are derived from the same epistemological theory, they can be directly compared. On the other hand, when they are derived from different epistemologies, there is no basis for direct comparisons. However, explanatory theories based on different epistemologies can and often do reinforce one another:

> The unencapsulated man would be particularly interested in what F. S. C. Northrop has called epistemic correlations. An epistemic correlation is simply an agreement reached through two or more valid approaches to reality.[11]

The problem involved in "validating" epistemologies is very like that of validating constructs when no criterion variable is available. Instead of cor-

[11] Royce, p. 196.

relating a predictor variable (e.g., scholastic aptitude) with a criterion variable (e.g., graduating grade point average), one must depend upon the *convergence* of a number of tests purporting to measure the same construct (various tests of "attitude," for example). This is analogous to the procedure involved in "validating" epistemologies. If explanatory theories based on two or more epistemologies demonstrate considerable convergence, one is inclined to place more value on both the epistemologies and the areas of convergence.

This convergent validation of epistemologies and their attendant explanatory theories requires that research, regardless of the epistemology on which it is based, be conducted according to certain *generalized* standards or criteria not specific to any one epistemology.

In our opinion, Fisher's four criteria and Scheidel's criteria for the three phases of research constitute such generalized criteria. Further, if one takes the *generalized* form of the criteria of Delia and Grossberg as they are stated at the end of their rejoinder and ignores their assumption that "reality" can only be represented phenomenologically, then those two criteria can be added to provide a list that is commendably comprehensive, general, and useful in the process in which we are all engaged: convergent epistemic validation.